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SNAKE POISON.

THE most important class of chemical substances with which the physiologist has to deal is that of the proteids. Their importance arises from the fact that they form the most essential of the constituents of a diet, and the most constant and abundant of the materials obtainable from protoplasm and living structures generally. In spite of this, however, we know practically nothing of their chemical constitution. The physical properties of the proteids, their identification by chemical tests, their subdivision into classes according to their solubilities, and the products of their decomposition have all been pretty thoroughly studied; there also exist various theories of the way in which their molecule is built up; but there is nothing certain at present. One of the greatest feats in chemical science will be the synthesis of albumin; but until this is done our knowledge of that overwhelmingly important substance must remain nebulous, and advance in physiological chemistry be hindered.

Not the least strange of the many puzzling facts in connection with the proteids is that many of them are poisonous. The poisonous proteids are not distinguishable by any well-marked chemical or physical properties from the non-poisonous or food proteids. When the idea of a proteid poison was first mooted it was received with incredulity; and it was suggested that the real poison was something adherent to the proteid, and if the proteid had been

prepared in a pure condition it would be found to possess no toxic properties. This hypothesis may be correct, for the methods at present in vogue for obtaining pure proteids leave much to be desired. These methods, however, improve year by year; but as they improve, the toxic power of the poisonous members of the albuminous group does not diminish, and it appears more and more certain that it is the proteid itself which is the poisonous agent.

Proteid poisons have been obtained from both the vegetable and animal kingdoms. Thus among those obtained from plants, one may mention the proteids obtained from jequirity seeds, the proteid associated with or identical with the ferment papain of the papaw plant, and lupino-toxin from the yellow lupin.

The most important of the animal proteid poisons are snake poison; the proteids in the serum of the conger eel and other fish; and proteid poisons found in certain spiders.

Poisonous proteids are also formed during ordinary digestive processes in the alimentary canal of every one of us from the proteids taken in as food. The peptones and the proteoses or albumoses (intermediate products in the process of hydration of which the terminal product is peptone) are fairly powerful poisons. 0·3 gramme per kilogramme of body weight injected into the blood will kill a dog, producing a loss of coagulability of the blood, a fall of blood pressure, a stoppage of secretions, and ultimately death by cessation of respiratory activity. Normally animals are protected from this poison by the lining membrane of the alimentary canal, so that no proteose or peptone is found in blood or lymph even during the most active periods of digestion. The cells of this membrane possess many remarkable properties, but one of the most important is this power of regenerating albumin from peptone.

Allied to the albumoses of ordinary gastric activity are the similar products produced by bacteria. The way in which bacteria produce disease has long been a matter of dispute, but the problem appears to be approaching solution. Pathologists have at last turned their attention to the chemi-

cal side of the question, and shown that whereas in some cases the poisons produced by the growth of micro-organisms are alkaloidal in nature, in by far the greater number the toxic product is a proteid. The one which is best known, or at least attracted most attention, is the toxalbumose contained in Koch's tuberculin.

The foregoing list is far from complete, but one cannot conclude it without mentioning another class of proteid poisons: these are the nucleo-albumins obtainable by suitable methods from most of the cellular organs of the body. Originally discovered by Wooldridge they were named by him tissue-fibrinogens, because they possess the remarkable power of producing coagulation of the blood within the blood vessels of a living animal. A very small dose will kill a rabbit or a dog, and death is as a rule produced by extensive clotting within the vessels, especially in the veins. Under certain conditions, however, especially in the dog, they produce the opposite result, namely a loss of coagulability similar to that produced by peptone. Wooldridge termed this the "negative phase of coagulation".

A practical outcome of all this work is the discovery of alexines or protective proteids. These appear to belong to the nucleo-albumin class also. In small doses they confer immunity on animals to larger doses of similar poisons, and thus the long-hidden secret of the *modus operandi* of vaccination and other forms of protective inoculation is at last beginning to be unravelled.

The limits of the present article would, however, be far exceeded if one were to take up fully all the points hitherto alluded to, and follow them into the various scientific and practical channels into which they have led. I therefore propose in the remainder of this paper to consider in detail one class only of the poisonous proteids: this is one which to the Englishman is of theoretical interest only, but to many thousands of our fellow-creatures it possesses a deep practical importance also: the poisons in question are those which are secreted by snakes.

In so doing I shall allude chiefly to the most recent work on the subject by Dr. C. J. Martin and Mr. J. M'G.

Smith of Sydney, who have obtained results in the case of Australian snakes which corroborate those obtained by previous observers with the cobra and other venomous serpents.[1] Many of the paragraphs that follow are quotations from these papers.

A complete investigation into the subject of snake poison must attempt to answer three questions :—

1. What is the poison?
2. What is its physiological action?
3. How can one best prevent or counteract this action?

The majority of previous workers have begun at the wrong end; for out of about four hundred references consulted, over three hundred are to papers in which the authors answer to their own satisfaction the third question, and describe the beneficial results following the administration of some such potent drugs as ash tea or human saliva, and the utter and entire futility of the whisky or other treatment.

Martin has adopted another method, and his papers relate to the first two questions only. He is to be congratulated on his results, especially as the investigation was fraught with difficulties. It was impossible to procure the services of a professional snake catcher, and so it was necessary for him to do all the work himself. As he puts it, it was also necessary to overcome that dislike and dread of the serpent which is instilled into the youthful intelligence at an early age in every Christian land.

The method of obtaining the poison was an ingenious modification of that adopted by the Indian snake men. The yield of poison per bite was very small, and so considerable time and patience were consumed in getting enough material to work with.

The small quantity secreted is apparently amply atoned for by quality, the minimal fatal dose per pound weight being considerably less than that given by the Indian Snake Commission for the cobra. Some idea of this virulence

[1] The Venom of the Australian Black Snake (Pseudechis porphyriacus), by C. J. Martin and J. M'Garvie Smith, *Proc. Roy. Soc. N. S. Wales*, Aug. 3, 1892; and *Journal of Physiology*, vol. xv. (1893), p. 380.

may be gathered from the fact that one-thousandth part of a grain invariably kills a rabbit of five pounds weight in about a hundred seconds.

This extraordinary toxicity becomes more astounding still when we consider that the poison is a proteid undistinguishable by chemical methods from those daily used as food by all of us.

The question of chemical constitution we take next. The first investigation into the chemistry of the snake poison of any importance was by Prince Lucien Buonaparte on the poison of an adder in 1843. He found that the activity of the poison was associated with that portion precipitated by alcohol; and he gave the name "viperine" to this precipitate. Dr. Weir Mitchell next turned his attention to the subject about 1860; and he is essentially the founder of our present knowledge concerning snake poison. Crude as were the methods of animal chemistry in his day, they nevertheless led him to the right conclusion that the toxic principle of the venom is albuminoid in nature. He termed it "croatalin" in the case of the rattlesnake. From that time till 1886, in conjunction with Reichert, he continued his work, and confirmed his general conclusion in the case of other North American snakes. About 1871 the Indian snakes received their share of attention; and the names of Sir Joseph Fayrer and Dr. Lauder Brunton are associated with valuable researches concerning the venom of the cobra, kraits and the Indian viper.

In 1883 Wall, in 1886 Wolfenden, and in 1893 Kanthack published most instructive contributions to our knowledge of cobra venom; the improved methods of chemical physiology enabling them not only to identify the poison as a proteid, but to show that the variety of proteid present is an albumose. Two observers only of importance have described poisons other than proteid in snake venom: one of these was Gautier, who regarded the venomous principle as alkaloidal; and the other, Winter Blyth, who gave the name "cobric acid" to a highly poisonous crystalline substance he claimed to have separated from cobra venom. Recent work has failed to substantiate these results, and alkaloids when

present at all (and they are generally absent) are non-poisonous ones.

In the researches on the venom of the Australian black snake, Martin and Smith found it necessary to exclude various classes of poisons, as well as to determine positively the nature of the venom. They excluded in the first place by appropriate experiments the presence of micro-organisms, ferments, alkaloids, ptomaines, and crystalline acids.[1] In the second place they showed that the poison was a proteid. The methods for the separation of proteids from one another are highly technical. It will therefore be sufficient to say that the manipulations were of the most recent and perfect kind, and pass to the results obtained. In the proteid mixture three proteids were obtained: one an albumin, and the other two albumoses. The albumin is not virulent, but the two albumoses (corresponding to proto- and hetero-albumoses of Kühne) are extremely poisonous. They each have the same physiological action, and this is the same as that produced by the venom itself. The venom can be momentarily boiled without impairing its activity, but prolonged boiling for days destroys its virulence.

The action of the poison may be described under two heads, (1) local and (2) general effects. The most marked of the local effects is œdema; the general symptoms consist of twitching and convulsions in non-lethal doses. A fatal dose kills within a few seconds or minutes. There is also a peculiar effect on the blood, which I propose to deal with more in detail immediately.

The conception put forward of the formation of these albumoses is the following:—

The cells of the venom-gland by a vital process exercise a hydrating influence on the albumins supplied to them by the blood, the results of which influence are the albumoses found in the venom. The difference between this process and digestion by pepsin or by anthrax bacilli is that the hydration stops short at the albumose stage, and is not continued so as to form peptone or simpler nitrogenous pro-

[1] A questionable trace of an organic acid found did not possess toxic properties.

ducts like leucine, tyrosine or alkaloids. Gland epithelium is certainly capable of exercising such a hydrating influence; the conversion of glycogen into sugar by the liver cells is one of the best known examples.

The following table, somewhat altered from Dr. Sidney Martin's Goulstonian lectures to the Royal College of Physicians, London (1892), illustrates the analogy between various hydration processes, proteid being in all cases the material acted on.

Primary Agents.	Ferment.	Products.	
		Albuminous.	Nitrogenous, but not Albuminous.
1. Epithelial cell of gastric glands.	Pepsin.	Albumoses. Peptone.	Pepto-toxine. (?)
2. Epithelial cell of pancreas.	Trypsin.	Globular-like substance. Albumoses. Peptone.	Leucine, tyrosine, aspartic acid, ammonia.
3. Bacillus anthracis.	None yet found.	Albumoses. Peptone.	Leucine, tyrosine, and an anthrax alkaloid.
4. Bacillus diphtheriæ.	Ferment not yet named.	Albumoses.	Organic acid of doubtful nature.
5. Epithelial cell of snake's venom-gland.	None yet found.	Albumoses.	Trace of organic acid. (?)

I have left to the last the effect of snake venom on the blood, because it opens up other questions, and has no particular bearing on the general aspect of the subject of proteid poisons. Fontana, more than a hundred years ago, noticed that the blood remained fluid in animals dead of viper bite, and Brainard, writing forty years back, states that when death occurred *immediately* in animals bitten by rattlesnakes the blood was found at the *post-mortem* examination to be clotted; but if some time elapsed before the animal succumbed, the blood remained fluid in the vessels. The continued fluidity of the blood has since then been noted by numerous observers in the case of various snakes.

Martin made most of his observations on dogs, but obtained confirmatory results on other animals (cats and rabbits). He found that different doses produced different results. Immediately after the introduction of the venom the coagulability of the blood increases; and this increase in the case of moderate or large doses (more than 0·0001 gramme per kilo. of body weight) culminates in intravascular clotting of greater or less extent. The injection of smaller doses produces a transient phase of increased coagulability, but after two minutes this is succeeded by a "negative phase"; the blood drawn either fails to clot at all, or does so only after the lapse of several hours. The thrombosis occurs more readily in venous than arterial blood, and is frequently confined to the portal area.

These results show a great resemblance between the action of the venom and that of tissue-fibrogen or nucleo-albumin. The effect of diminished coagulability is not unexpected, seeing that the principal substance in the venom is albumose. But the minuteness of the dose necessary is very striking and distinctive.

The question arises, does the poison contain nucleo-albumin? A nucleo-albumin is a proteid united to a substance rich in phosphorus, called nuclein. It can be detected by the fact that artificial gastric digestion dissolves the proteid and leaves the nuclein as an insoluble residue. This residue must then be examined for phosphorus. Snake venom contains no nucleo-albumin; and its action not only opens up a novel aspect of the subject of snake poisoning, but also sheds light on the vexed problem of blood coagulation.

The smallness of the dose suggests that the injected material does not contribute itself to fibrin formation. Probably it acts by producing disintegration of the cells in proximity to the blood stream, such as the endothelial cells lining the vascular system. If it thus liberates nucleo-albumin from these the conditions would be practically the same as if this toxic agent were injected from without. The venom *is* capable of playing havoc with these cells. This was originally shown by Weir Mitchell and Reichert. These

authors moistened the mesentery of a cat with a solution of rattlesnake venom, and observed under the microscope the rapid formation of extensive capillary hæmorrhages. Martin repeated these experiments, using black snake venom; and although the action of this poison is less rapid than was the case in Mitchell's experiments, the results were identical.

Whether the venom causes any destruction of the white blood corpuscles is doubtful. These are massed together in such a way that their enumeration becomes a difficult matter. The plasma is stained with hæmoglobin, indicating that a slight solvent action on the red corpuscles has taken place. This, however, is not a distinctive action of snake venom. It is, moreover, well known that substances like distilled water which produce extensive disintegration of blood corpuscles within the blood stream never produce thrombosis; so that, even if the venom produces a disappearance of the leucocytes, that would in itself be insufficient to cause intravascular coagulation.

From this summary of the subject of snake poison one sees how much of interest exists in such researches. They open up fresh questions in wide and important general subjects, two of which, namely, blood coagulation and the poisonous nature of certain proteids, it has been the object of this paper specially to emphasise.

W. D. Halliburton.

ALGÆ AS ROCK-BUILDING ORGANISMS.

FROM the point of view of a botanist the phycological records of the rocks are exceedingly disappointing. The Lower Palæozoic strata have afforded such genera as *Nematophycus* and *Pachytheca*, which may perhaps be regarded as extinct forms of algæ, but cannot as yet be assigned to any definite position in that class of plants. The tissues of these two genera have been described by various observers, and botanists have exercised considerable ingenuity in their speculations as to the exact position of both fossils in the vegetable kingdom.

Apart from *Nematophycus* and *Pachytheca* our knowledge, such as it is, of fossil algæ is limited to structureless specimens of various forms and sizes, which have been referred to algal genera on the evidence of any superficial resemblance to the bodies of living forms.

The calcareous algæ afford a notable exception to the general rule as regards the unsatisfactory state of fossil phycology. Their hard parts have left well-preserved remains in rocks of different ages, and have supplied trustworthy data for the study of extinct forms. In addition to the calcareous algæ, and a very few isolated examples of non-calcareous genera, there are but few records which can be utilised in phylogenetic studies. When we approach the subject of fossil algæ from a geological standpoint, we are confronted with a mass of facts, collected for the most part in recent years, which reveal the great importance of algal remains as rock-building agents. It would be superfluous to draw attention to such diatomaceous deposits as occur in the Cretaceous and Tertiary formations, or to describe the various conditions under which diatoms are now building up siliceous rocks. If we accept Heer's genus *Bactryllium* as an unusual type of diatom we may extend the geological history of these rock-builders to Keuper and Muschelkalk times, but from older strata trustworthy records have still to be sought for.

At the present day we find such genera as *Lithothamnion*, *Lithophyllum* and others taking a more or less prominent part in rock construction; in some cases adding their calcareous skeletons to the accumulating masses of coral reefs; under other conditions spreading over the ocean floor in high northern latitudes.[1] The thick masses of limestone of various geological ages containing numberless specimens of calcareous Florideæ testify to the widespread occurrence of the same and similar forms in the ancient seas. Other strata are obviously built up very largely of calcareous algæ of a simpler type, and reveal the past history of numerous genera which must be placed in the same group with *Cymopolia* and other recent forms. Not only do many rocks supply clear and undoubted proof of their partial or complete development from the accumulation of calcareous plants, but in many cases there are striking facts to hand which point to a phytogenetic origin for strata without any present traces of organic structure.

OOLITIC ROCKS AND ALGÆ.

Oolitic structure characterises many rocks of Palæozoic, possibly also pre-Palæozoic, Mesozoic and Tertiary age, and is of common occurrence in recent calcareous deposits. The best known examples of oolitic rocks are those which form part of the Oolitic series in the Jurassic formation. A typical oolite consists of "rounded particles varying in size from a pin's head to a pea,"[2] with usually some foreign body as a central nucleus round which the calcareous substance has been deposited. The microscopic structure and mode of origin of oolitic sediments were made the subject of examination by Dr. Sorby some years ago, and it was generally agreed that the carbonate of lime of each grain had been deposited layer by layer round a grain of sand, shell fragment or other nucleus as it was carried to and fro in an eddying current. At all events the formation of oolitic grains was referred to some essentially inorganic process.

[1] Kjellman, p. 66. [2] Green, p. 281.

The author of a recent paper on oolitic structure concludes with the following words: "In concluding this paper I should like to say, first, that it has been my object to produce evidence that oolitic structure is not always of concretionary origin. That it is all organic I am not prepared to maintain, but it may be."[1] The change of view thus expressed by Wethered is the outcome of a more intimate acquaintance with the minute structure of oolitic grains. In sections of oolitic rocks from various Palæozoic and Mesozoic horizons it has been conclusively demonstrated that a small tubular structure is of very frequent occurrence in the individual calcareous grains. To this tubular fossil has been assigned the name *Girvanella*. The name was chosen from the discovery of this structure in Ordovician rocks in the Girvan district of Scotland. Nicholson and Etheridge have given the following diagnosis of the genus: "Microscopic tubuli, with arenaceous or calcareous (?) walls, flexuous or contorted, circular in section, forming loosely compacted masses. The tubes, apparently simple cylinders, without perforations in their sides, and destitute of internal partitions or other structures of a similar kind."[2] Without expressing themselves very decidedly as to the systematic position of the genus the authors are inclined to regard it as a rhizopod. Since its discovery in the Girvan rocks *Girvanella* has been repeatedly found by other observers in strata of different ages. The careful researches of Wethered have been especially important in drawing attention to the widespread occurrence of this problematical organism. Possibly it may eventually find a definite place among fossil algæ, but at present it is safer merely to suggest such a systematic position rather than to attempt a more precise determination. The discovery of this new structure in oolitic grains has naturally called forth numerous and diverse expressions of opinion as to its probable nature, but it is unnecessary to attempt a detailed examination of such speculations.

In an interesting contribution from Prof. Nicholson,

[1] Wethered (2), p. 281. [2] Nicholson and Etheridge, p. 23.

"On certain anomalous organisms which are concerned in the formation of some of the Palæozoic limestones,"[1] the author adheres to his original opinion as to the affinities of *Girvanella*. Two figures accompanying his paper afford a good idea of the nature of this doubtful genus: in one a section of the Craighead (Ayrshire) limestone is shown to contain several small rounded or irregular nodules suggestive of oolitic structure; the second figure exhibits the characteristic tubular structure of the *Girvanella* clusters. Nicholson also notes the occurrence of the same fossil in certain Lower Palæozoic rocks of North America. In 1889 Wethered discovered the same tubular structure in Jurassic pisolites;[2] he examined microscopic sections of Coralline Oolite from the neighbourhood of Weymouth and of Pea Grit from the northern Cotteswolds. In both cases the calcareous spherules were found to have a central nucleus surrounded by rudely concentric layers of innumerable minute tubuli exhibiting "vermiform twistings and turnings". Such a microscopic structure in the pisolite grains was naturally regarded as entirely opposed to the ordinary view as to the concretionary origin of oolitic structure. Each granule was shown to have resulted from the growth of some organism round a central nucleus; the organism itself Wethered referred to Nicholson's genus *Girvanella*. By his subsequent investigations Wethered has considerably extended the range of this fossil, and further evidence has been accumulated as to its frequent connection with oolitic structure. An oolitic structure is fairly common in the Carboniferous limestone of Gloucestershire, and in some localities *Girvanella* has been detected in the individual spherules, the oolitic grains occasionally having a nucleus in the form of a foraminiferal shell fragment, but more frequently in these older rocks the central calcareous fragment has been crystallised as calcite.

The tubules of this Carboniferous species, *Girvanella Ducii*, are wrapped round a central nucleus, and impart to the spherules an appearance suggestive of concentric layers

[1] Nicholson, p. 23, fig. 5, A and B. [2] Wethered (1).

of growth. In another species from the same rocks, *G. incrustans*,[1] the narrow tubules form a crust round a calcite nucleus. The Jurassic Oolites have afforded such species as *G. pisolitica*, *G. intermedia* and *G. minuta*. In all these cases the organism consists of tortuous and aggregated tubules exhibiting certain variations in size and manner of arrangement. The frequent association of this structure with oolitic grains leads Wethered to the conclusion that the spherules of such rocks must be referred rather to an organic than to a concretionary origin.

Dr. Hinde has called attention to the occurrence of *Girvanella* in Ordovician rocks in the province of Quebec, and to the discovery by Bornemann of similar structures in Cambrian rocks of Sardinia.[2] Bornemann has instituted a new genus *Siphonema*[3] for certain incrusting calcareous algæ consisting of minute tubular cells. He compares the Sardinian genus to the recent *Scytonomaceæ*, and expresses the opinion that in all probability many of the oolites from various geological horizons will be found to supply similar proof of their organic origin. The nature of these Sardinian fossils and their concentric disposition round fragments of shells, crinoids or corals are very like that of *Girvanella*, and there can be little doubt that Bornemann's genus is the same as that previously established by Nicholson and Etheridge.

The species of *Siphonema*, *S. incrustans*, which occurs in the Sardinian rocks has also been found by Bornemann in a limestone boulder which probably came from Baltic rocks of Silurian age. Additional information has been collected by Wethered as the result of his microscopical researches on the structure of Oolitic rocks from the Cotteswold Hills.[4] In a still more recent communication to the Geological Society the same author refers to the occurrence of *Girvanella* tubules in Devonian limestones from South Devon;[5] and on another occasion the existence of *G. pro-*

[1] Wethered (2), p. 280, pl. xi., figs. 1 *a*, *b*.
[2] Wethered (2), p. 282.
[3] Bornemann (1), pl. ii.
[4] Wethered (3).
[5] Wethered (4).

blematica, associated with other species of the same genus, is recorded in the Wenlock limestones of May Hill.

Wethered considers that the most interesting result of his microscopical examination of the Wenlock limestone has been the discovery of "new and interesting forms of *Girvanella*, and the fact that this organism has taken so important a part in building up limestones".[1] Aggregations of *G. problematica* were met with in rocks of the same age near West Malvern and in sections of limestone from Ledbury. In summarising his results Wethered makes certain suggestive remarks as to the method of formation of some of the Wenlock beds in which the tubular organism is so abundant; he writes as follows: "Fragments of organisms were deposited on the sea-floor, around these the *Girvanella* tubules collected, sometimes entirely enclosing a fragment with a crust of tubules, and thus giving rise to a spherule or granule, the determination of which depended on the shape of the fragment enclosed".[2]

Without attempting any complete discussion of the references by other writers to this important and almost ubiquitous fossil, we must note the fact, recently pointed out by Rothpletz,[3] that Nicholson has declared himself favourable to the inclusion of *Girvanella* among (calcareous) algæ.

It has been suggested by Clement Reid[4] that possibly the calcareous tubules of *Girvanella* may be looked upon as cylindrical sheaths in which small filamentous algæ became enclosed. If such an explanation be accepted we can hardly speak of the genus as a true calcareous alga, but as an alga which was able to bring about a deposition of carbonate of lime outside itself, and not actually in its own cell walls.

Another author, Dr. Brown,[5] has recently lent his support to the opinion, previously expressed by Rothpletz and others, that *Girvanella* must probably be placed among calcareous algæ in the family *Siphoneæ*.

[1] Wethered (5), p. 239.
[2] Wethered (5), p. 246.
[3] Rothpletz (1), p. 302.
[4] Wethered (5), p. 248.
[5] Brown, p. 203.

How far do other observations on the origin of oolitic grains support the contention that *Girvanella* has in many instances been instrumental in producing such rock structure? On the shores of the Great Salt Lake of North America there are accumulations of snow-white calcareous grains which the waves have washed up on the beach; these minute spherules correspond in the arrangement of the carbonate of lime, and in external form, to typical oolitic grains. The nature of this modern oolitic material has recently been made the subject of examination by Rothpletz.[1] On dissolving the carbonate of lime he finds that each grain shows a residue of algal cells, some of which he compares to *Glœocapsa*. Cells of this genus and of *Glœotheca*, surrounded by the usual gelatinous sheaths, occur as a coating to the calcareous spherules whilst still in the waters of the lake. In all probability the Salt Lake oolite is a product of algal life. Rothpletz's researches in oolitic structure lead him to the conclusion that at least the majority of marine calcareous oolites, which exhibit a regular zonal and radial structure, must be looked upon as the results of algal life. Another example of oolitic structure closely connected with the growth of algæ may be quoted from an important work by Walther. This author describes oolitic grains from the shores of the Red Sea which have a central nucleus of quartz, felspar or garnet surrounded by carbonate of lime enclosing algal cells and branched tubular structures.[2]

Finally, Bleicher[3] has discovered organic matter in Jurassic oolitic grains from Lorraine, and in some cases this takes the form of a tubular network of algal-like filaments. Here, again, we have an organism, and presumably an alga, entering into the composition and concerned in the formation of oolitic grains.

It appears from the researches of Rothpletz that in some cases another form of alga closely allied to *Girvanella* has played an important part in the production of oolitic structure; this is the genus *Sphærocodium*, which he

[1] Rothpletz (2). [2] Walther (2), p. 482.
[3] Bleicher (1) and (2).

describes from the Upper Alpine Trias and places in the family *Codiaceæ*.[1] It occurs in small spherical or oval aggregations which were formerly regarded as concretionary oolitic spherules. In the Raibler and St. Cassian beds there occur bands of limestone to a large extent built up of these *Sphærocodium* spherules; Rothpletz figures and describes this alga as *S. Bornemanni.*

According to Bornemann[2] a fossil genus of the *Rivulariaceæ* has also been responsible for oolitic structure in some cases; he institutes the genus *Zonatrichites* for this form, and gives the following definition of it: "A fossil calcareous alga with radially arranged filaments, forming hemispherical or kidney-shaped layers, growing on or enclosing other bodies. Parallel or concentric zones are seen in cross section, formed by the periodic growth of the alga, the older layers serving as foundations on which the younger filaments grow in radially arranged groups."

In an appendix to Bornemann's paper the question of oolitic structure is dealt with, and descriptions are given of microscopic preparations of oolitic rocks showing the nature of the algal elements in the individual grains.

In his paper on rock-building algæ in the Swiss Alps, Früh[3] calls attention to the association of *Zonatrichites* with oolitic structure.

As another example of recent algæ as oolite-builders, reference should be made to Weed's description of the travertine deposits at the Mammoth hot springs in the Yellowstone Park.[4]

Enough evidence has been adduced, without citing further examples, to show that there is reason to believe that algæ have a much greater claim to the attention of geologists as possible agents of rock construction than has generally been admitted.

[1] Rothpletz (1), pl. xv., figs. 5 and 6, etc.

[2] Bornemann (2), pls. v. and vi.

[3] Früh.

[4] Weed, p. 643.

CALCAREOUS ALGÆ OF THE CLASS FLORIDEÆ.

The Miocene Leithakalk of the Vienna Basin has long been quoted in text-books as a rock largely composed of the genus *Lithothamnion*. This characteristic fossil was originally described by Reuss as *Nullipora ramosissima* and placed by him in the category of fossil animals. The genus *Nullipora* has been used in an extremely wide sense, embracing organisms which are for the most part members of the vegetable kingdom, but including others which must still be retained as animals. An important contribution to the study of the Leithakalk appeared in 1858 by Unger.[1] He refers to the discovery by Philippi, in 1857, that several of the so-called Nullipores had been erroneously included in the animal kingdom, and goes on to describe in detail the structure of *Nullipora ramosissima* from the Vienna limestones. Unger gives descriptions and figures of the genera *Lithothamnion* and *Lithophyllum*, and recognises the important *rôle* of the fossil forms of these and similar algæ in the formation of limestones.

The microscopic examination of the Leithakalk has been considerably extended by Gümbel, who gives a number of interesting facts as to the frequent occurrence of calcareous algæ in limestones of different ages and localities.[2]

Specimens of *Lithothamnion* are described from Jurassic rocks near Neukirchen, the Maestricht chalk, Pisolites of the Paris Basin, etc.

With a view to throwing fresh light on the geological action of calcareous rock-forming algæ, Walther availed himself of the facilities afforded by the Biological Station at Naples to make a special study of living rock-building plants. He examined the Secca di Chiaja, the Secca della Gajola and the Secca di Penta palumno, and investigated the formation of calcareous deposits at present in operation in those submarine plateaux.

The Secca della Gajola is described as a typical example of a recent algal deposit; it is situated about one kilo. from the coast and 30 m. below the surface of the water.[3]

[1] Unger. [2] Gümbel. [3] Walther (1).

Numberless nodules of *Lithothamnion ramulosum* and *L. racemus* are brought up in the dredgings from this calcareous bank; also foraminifera, gasteropods, diatoms and other organisms. By the action of percolating water the *Lithothamnion* structure is gradually obliterated, and the calcareous mass becomes a structureless limestone.

Walther applies his knowledge of this recent algal deposit to the examination of a Tertiary "Nulliporenkalk" near Syracuse. In many parts of this formation there occur well-preserved specimens of *Lithothamnion*, but in others a gradual obliteration is observed of all plant structure until the rock becomes entirely structureless. A similar instance of structureless limestone is described from the Lias of Todten Gebirges; the strata consist of coral rock, detrital calcareous deposits, and, associated with these, masses of limestone in which microscopic examination fails to detect either vegetable or animal structure. These structureless beds are considered to have been *Lithothamnion* banks from which percolating water has removed all trace of algal cells. It is suggested that the infiltrating water was supplied by the *Lithothamnion* thallus with the necessary amount of carbonic acid, and was thus enabled to remove all direct evidence of the existence of calcareous algæ. In connection with this solvent power of the water Walther asks the question: "What becomes of the plant cellulose in the process of fossilisation?" An instructive comparison is made between the chemical composition of compact *Lithothamnion* masses from the Secca di Penta palumno and the Tertiary *Lithothamnion* limestones in the neighbourhood of Syracuse; in the former the $CaCo_3$ reaches 86°/$_\circ$, and the organic substance 5°/$_\circ$; in the latter the $CaCo_3$ reaches 98°/$_\circ$, and organic substance 0·28°/$_\circ$. The organic substance of the algæ became chemically altered in the Syracuse beds, and in the course of such changes carbonic acid was evolved; this was readily taken up in solution by percolating water, which was thus supplied with the means of obliterating all traces of *Lithothamnion* structure.

Thus it is shown that in masses of calcareous algal remains there is an "endogenous source" of carbonic acid

which may frequently result in the removal of all signs of phytogenetic origin. On the other hand, in many calcareous beds the percolating waters have not found the same amount of carbonic acid, and their solvent power has not been sufficient to effect the destruction of the organic remains from which the strata have been formed. If the calcareous deposits are protected from the circulation of carbonated water by overlying impervious beds, the organic structures would not be removed. It would seem, therefore, that under certain circumstances, in which calcareous deposits are freely exposed to infiltrating water, there is a much greater probability of all structure being removed in the case of those formed from calcareous algæ than in deposits which are not of phytogenetic origin. Walther's researches are of extreme importance from a geological point of view; they at least show that the study of living calcareous algæ may possibly lead the petrologist to approach from a new standpoint the difficult question of the genesis of structureless limestones.

In two recent numbers of the *Geological Magazine* Brown [1] has given some account of another calcareous organism, *Solenopora*, which his investigations compel us to annex as a member of the *Florideæ*. It has a wide geographical distribution, and its geological range extends from Ordovician to Jurassic times. The author describes several species from various districts and horizons, and concludes his remarks with a discussion of the botanical position of the genus. He compares it with *Lithothamnion*, *Corallina*, and other calcareous genera: "the form of the cells and cell walls, the method of increase, and the arrangement of the tissue cells in the various species of *Solenopora* bear strong evidence of relationship between that genus and the calcareous algæ". Additional evidence of taxonomic value is derived from the occurrence of reproductive organs in some few specimens. There can be little doubt that *Solenopora* should be included among the rapidly growing list of fossil calcareous algæ which must claim the atten-

[1] Brown, p. 200, pl. v.

tion of phycologists, as well as increased recognition on the part of geologists.

A recent discovery of *Lithothamnion* in a cutting at the mouth of the River Liffey deserves a passing notice. In deepening the extremity of a well in 1871-72 several shells which had been dug up in the process of excavation were found to be grown over with some calcareous organisms. These encrusting growths were determined to be species of *Lithothamnion*, very like *L. polymorpha* and *L. fasciculatum*, both of which are still living in the Bay of Dublin, but no longer exist in the Liffey.[1]

ALGÆ AND HOT-SPRING DEPOSITS.

In an exhaustive monograph on this subject Weed expresses his belief that "the geological work of plant life has not been generally recognised, partly because it is less conspicuous, and partly because the absence of organic remains in many deposits formed in this way has prevented a recognition of the true origin of the rocks".[2] The able account by this geologist of the Yellowstone Park hot springs should have the effect of raising the importance of the lower forms of plant life as geological agents.

Records of algæ in the waters of hot springs have been supplied by various observers, and the power of such plants to withstand high temperatures has been long known. Weed has done good service in calling attention to the consequences of such algal life in thermal waters. The exact nature of the vegetation in the Yellowstone Park springs is often difficult to determine, the plants being frequently encrusted and hidden by such substances as sulphur, sulphate of calcium, oxide of iron, etc. In the district of the Mammoth hot springs, extensive masses of travertine or calcareous tufa have been gradually built up. The springs themselves vary in temperature from 80° to 165° F., and all have afforded numerous examples of actively living plants. The growths vary considerably in character and colour according to the temperature of the water; in rapidly flow-

[1] O'Reilly. [2] Weed, p. 619.

ing currents the plants assume a filamentous form, in quieter pools they are united in membrane-like sheets. Not infrequently the filaments are embedded in travertine with the tips alone projecting. The withdrawal by the plants of carbonic acid from the water causes a separation and precipitation of carbonate of lime, which gradually gives rise to fan-shaped or terraced masses of calcareous rock. About many of the springs there occur masses of gelatinous algal growths, consisting of successive membranous sheets; in these are embedded minute gritty particles which on further development become aggregated together in the form of small grains of carbonate of lime. These spherules afford another example of algal oolite. In the older layers the oolitic structure is masked by the cementing together of the separate pellets, and thus the gelatinous masses gradually pass into firm and more or less compact travertine. The travertine of the Mammoth hot springs which has not been the result of such organic agencies forms but a small part of the deposits. In describing the more fibrous part of the calcareous masses Weed refers to its obvious organic origin, and speaks of the fine algal threads which have been instrumental in its formation, "as if a skein of silk floating in the shifting currents of a stream were suddenly turned to snowy travertine".

The following causes are mentioned as having been concerned in the production of travertine deposits: Relief of pressure, diffusion of the carbonic acid by exposure to the atmosphere, evaporation, heating and the influence of plant life.

Turning to the siliceous springs of the Yellowstone Park region the same observer finds that much of the siliceous sinter has been formed by the growth of such plants as algæ and mosses. The brilliant tints of red, yellow and green are all produced by such living plants as have the power of separating the substance of the sinter from the hot siliceous waters.

In some places the algæ occur in the form of tough leathery sheets of gelatinous material, in others they appear as delicate skeins of white filaments. As in the

calcareous springs, here too a gradual transition may be traced from the living gelatinous growths to hard and compact sinter. *Calothrix gypsophila*, *Leptothrix* and other genera are recorded from these sinter-forming springs; such plants are spoken of as algæ, but probably they would be more fitly described as bacteria. Some forms of the siliceous sinter appear to have been mainly formed by a species of moss, *Hypnum aduncum* var. *grasilesiens* Br. and Sch.

The causes which have operated in the formation of sinter are summarised as follows: Relief of pressure, cooling, chemical reaction and evaporation.

This subject of sinter-building by low forms of plants was discussed at some length in an important communication from Prof. Cohn[1] in 1862, in which he demonstrated the importance of such forms in the deposition of the Carlsbad "Sprudelstein".

When we take account of the occurrence of algæ and bacteria in the hot springs of New Zealand, Iceland, India, the Azores, Malay Archipelago, Japan, America and other places, it must be admitted that Weed had good grounds for asserting that the geological work of the lower forms of plant life has not been generally appreciated.

SIPHONEÆ VERTICILLATÆ.

Did space permit some account might be given of the numerous calcareous algæ in the Triassic limestones of the Southern Alps. Such genera as *Dactylopora*, *Haploporella*,[2] *Gyroporella*, *Uteria* and many others were originally described as foraminifera by Carpenter, Gümbel and other writers. Gümbel included in the family *Dactyloyideæ*[3] a rich variety of forms possessing calcareous skeletons, and his papers contain valuable information as to the struc-

[1] Cohn:

[2] It should be noted that Rothpletz has recently pointed out that *Haploporella fasciculata* Gümbel is not an alga, but consists of echinoid spines (*Neues Jahrb.*, vol. i., 1891, p. 285).

[3] Gümbel, p. 253.

ture and distribution of living and fossil species. He refers to the occurrence of several genera and species from the Tertiary Paris Basin, the Trias of the Alps and Silesia, and from other geological formations. A few years subsequent to the appearance of Gümbel's monograph, Munier-Chalmas[1] made out a clear case for the removal of these so-called foraminifera to a new class of calcareous algæ to which he gave the name of *Siphoneæ Verticillatæ*. In this class he included fifty genera ranging from the Trias to the present day, all characterised by consisting of a simple or dichotomously branched frond with a tubular unicellular axis giving off verticils of branches. A useful account of these important plants will be found in Solms-Laubach's *Fossil Botany*;[2] his critical remarks carry the weight of an authority on recent calcareous algæ.

The recent researches of Bertrand and Renault point to the operation of simple forms of algal life in the production of such carbonaceous deposits as the boghead of Autun, Scotland and other districts; reference was made to the work of these authors in the first number of "SCIENCE PROGRESS".[3]

NOTE.—Since writing this article my attention has been drawn to a recent paper by Wöhrmann (Alpine and Ausseralpine Trias) in the *Neues Jahrbuch*, bd. ii., heft 1, 1894, p. 1. The author lays stress on the important share which algæ have undoubtedly had in the formation of thick masses of limestone and dolomite rocks; the Codiaceæ and Siphoneæ are the families which have been chiefly concerned in the building of these Triassic strata.

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[1] Munier-Chalmas. [2] Solms-Laubach, p. 37.
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THE BIOLOGICAL CHARACTERS OF BACILLUS TYPHOSUS (EBERTH) AND BACTERIUM COLI COMMUNE (ESCHERICH).

BACTERIOLOGY offers so many problems for the consideration of investigators in allied sciences that the remarkable growth of a considerable literature in connection with this subject is easily understood. Questions which ten years ago were prominent, such as the monomorphic or pleomorphic characters of bacteria, have to-day an interest which is secondary to their physiological and pathological behaviour. The study of any single micro-organism tends to confirm the belief in the flexibility of form which many saprophytic and parasitic bacteria may exhibit, and consequently investigations into the nature of the exchanges of material exhibited by bacteria appears in many cases to afford a safer guide for the determination of definite specific characters than a too rigid adherence to purely morphological features. Such considerations are especially evident in connection with the marked variability of such pathogenic forms as spirillum choleræ, bacillus typhosus or bacterium coli commune. The bacilli of cholera, according to the researches of D. D. Cunningham (1), show no fewer than eight distinct species which differ in form and cultural growth. Even if the contention that these species simply represent nutritive modifications of a single definite form (2) is sustained, it is abundantly clear that great morphological variation may occur. Friedrich (3) has undertaken a detailed study of cholera vibrios and has arrived at the conclusion that the species described by Cunningham are only multiple varieties of Koch's spirillum, which undergoes alterations in form which it is impossible to control; the most abnormal comma-bacilli may at any time take on the character which may be considered typical, that is the form originally described by Koch. A second distinct type of cholera vibrio exhibits longer and more delicate filaments which are frequently twisted into a spiral.

Sanarelli has shown that many varieties or races may connect these two types, and Metchnikoff (4) believes that these types are not constant forms but simply two races which can easily be transformed the one into the other by causing them to pass through leucocytes. Multiple varieties of the pneumococci have been demonstrated by Foà, and other micro-organisms have also been shown to possess great flexibility of form.

According to most bacteriologists the bacterium coli commune, described by Escherich (5) in 1885 as occurring in the excreta of children and animals, is a purely fæcal microbe identical with the form discovered by Emmerich (6) in Naples during the cholera outbreak of 1884, and considered by him to be the specific cause of cholera, a view which at once became untenable when it was shown that this micro-organism is constantly present in both the normal and abnormal contents of the bowel, and could also be isolated from air and putrefying liquids (7). It has been remarked that Escherich's bacillus is so ubiquitous and variable in form and cultural behaviour that at the present time it has usurped the position held years ago by bacterium termo. Still the chief interest of bacterium coli commune lies in its pathogenic character and in a morphological resemblance to the bacillus typhosus, which is so marked that the Lyons School hold that the two bacteria are simply harmless and harmful varieties of a common form. Since typhoid fever is undoubtedly propagated by contaminated water or milk, the importance of establishing definite criteria for proving the presence of specific pathogenic microbes in these liquids is evident, and as soon as it becomes possible to recognise the bacillus typhosus in dejecta with the same certainty as the tubercle bacillus can be demonstrated in sputa a great step will be accomplished in the diagnosis of a disease which in its early stages is not always easy of recognition. Aspiration of the spleen and cultivation of the bacillus typhosus from blood obtained by this procedure has been advocated by Redtenbacher (8) and others as a means of diagnosis in doubtful cases of typhoid fever ; and although this operation is stated to be unattended with bad results,

observations made upon excreta would certainly be more practicable. With the object of establishing a sound differential diagnosis between bacillus typhosus and other micro-organisms which are possibly closely allied, a considerable amount of work has been performed, some of which will here be briefly mentioned.

Purely morphological considerations are certainly insufficient to differentiate bacillus typhosus from many other microbes. This bacillus is met with in the tissues in the form of a short, relatively thick rod with rounded ends, which when cultivated frequently grows into threads with a much smaller diameter. Escherich's bacterium and its numerous varieties, the numberless typhoid-like bacilli isolated from water and the bacillus levans recently described by Lehmann (9) may all as far as shape is concerned be mistaken for the typical virulent specific microbe when isolated from the spleen of a typhoid patient. According to Luksch (10) bacillus typhosus exhibits much more lively movements than the bacterium coli, and micro-photographs show that the former of these is provided with 8-12 cilia, which are attached both to the sides and ends of the bacillus, while the latter only possess 1-3. Neither stains with Gram's method and no spore formation takes place. Shortly after the discovery of the bacillus typhosus a sporing stage was described by Gaffky, but this observation has not been confirmed. Recently Almquist (11) considers that spore formation can take place both in Eberth's and Escherich's bacteria. His method for demonstrating this consists in filling Erlenmeyer's or Pasteur's flasks with damp filter-sand or sand soaked with manure. This medium is sterilised and then inoculated with a bouillon culture of the microbe. The flasks are then placed for at least a month in an ice chest. By this time all the vegetative forms have disappeared and minute bodies about $\cdot 5\mu$ are found in the culture medium. He regards these bodies as spores, and describes them, when examined in a hanging drop of bouillon, as growing into rods which again break up into spores. How far these bodies deserve this name is doubtful, and Ilkewicz (12), who by a modification of Kolossow's method has succeeded in staining

the spores of anthrax bacilli so that these appear to each contain a nucleus, finds that under similar treatment the typhoid bacillus and bacterium coli commune only exhibit a simple beaded appearance which has nothing of the nature of spore formation.

Since morphological characters and micro-chemical reactions do not furnish sufficient criteria for the determination of the identity of bacillus typhosus, considerable attention has been given to the behaviour of this bacillus upon various culture media; this microbe is not capable of transference to animals, and therefore this method of diagnosis, which is absolute for the bacteria of such diseases as anthrax or glanders, is excluded. Some observers have published cases of an experimental typhoid fever which follows the introduction of bacillus typhosus into the peritoneal cavity of mice and guinea pigs, but the condition which is established by this method appears to be due to an intoxication with the products of the metabolism of the bacterial culture rather than to a distribution of the microbes in the infected animals that is in any sense comparable with the mode in which the specific bacilli of typhoid fever are found to spread in the human body. In connection with this question the recent papers of Sanarelli (13) and others are of great interest.

Some few years ago the character of bacterial growths on potatoes, in bouillon, gelatine, agar, together with plate cultivations, played an important part in affording differential characteristics for microbes, and it is beyond question that in some cases a determination of a form is possible by these means alone. Since Gaffky (14) first drew attention to the remarkable growth which bacillus typhosus exhibits on potato this has been held to be typical of this specific microbe. A slightly acid reaction of the medium, however, appears necessary, and E. Fraenkel and Simmonds were the first to point out that on many varieties of potato an obvious dirty grey growth occurred, instead of the almost invisible film which is so peculiar. Many observers, and among these Germano and Maurea (15), consider the growth on potato of little value as a diagnostic sign; and the observations of Fuller (16) are interesting, since he isolated no fewer than

five kinds of bacilli from the Merrimac which exhibited a growth on this medium exactly of the same character as that of the typhoid bacillus. Dunbar (17) considers that culture methods generally only complicate the solution of the problem of differential diagnosis of Eberth's bacillus from the bacterium coli commune, and the attempts made in this direction by Uffelmann (37), Vincent (41), Gasser (42), Holz (39), Parietti (40), Chantemesse and Widal, Laruelle (38) and Wurtz (35), which are all culture methods, are, according to this observer, insufficient. Quite recently Inghilleri (43) regards the growth of these bacteria upon amygdalin-bouillon as differential. Bacillus typhosus does not as in the case with the colon bacterium act like emulsin and decompose the glucoside with production of hydrocyanic acid. This effect however is certainly accomplished by other micro-organisms, as Telmi and Montesano have shown; the blood of cholera patients and the dejecta of those suffering from typhoid fever also possess the same property.

Recent papers have again brought into prominence the possibility of establishing a diagnosis between the above bacteria by culture methods. Buchner (18) first pointed out the anti-bacterial action of formalin, though a note on this fluid had been given by Löw (19). This agent has been employed by Schill (20) in order to obtain a differential culture medium. He describes a water-bacterium which occurs with Escherich's bacterium in spring and canal water. This resembles bacillus typhosus since it does not coagulate milk, but resembles bacterium coli commune since it develops gas in sterile bouillon and grows on potato with the production of a well-marked, yellowish green crop. To determine the nature of this water bacterium comparative experiments were made to differentiate bacillus typhosus from bacterium coli commune. A formalin-bouillon 1 : 7000 is prepared, and this is inoculated. When working with dejecta it is found that if the bouillon clouds this effect is due to the presence of Escherich's bacterium and is a valuable diagnostic character since bacillus typhosus causes absolutely no turbidity. This fact does not, however, definitely establish the presence of this microbe, since probably other water bacteria

which have the appearance of typhoid bacilli will not grow. At all events the turbidity of formalin-bouillon is decisive against the presence of bacillus typhosus.

Since formalin is only a saturated solution of formyl aldehyde in water, Schill has also examined the anti-bacterial action of this gas. He states that exposure for seventy minutes not only inhibits but kills bacillus typhosus, while bacterium coli commune and typhosus-like water bacteria preserve their vitality after an exposure of over two hours; a longer period inhibits their activity though this is recovered in twelve hours when the action of the vapour is abolished. Under no circumstances does Eberth's bacillus recover after similar treatment. Interesting as these details are, they do not really mark an advance on earlier methods for differentiation such as those employed by Chantemesse and Widal (21), by Parietti and others, where cultivations upon media such as carbol-bouillon, methyl-violet gelatine or fuchsin-agar are largely relied upon as a means of diagnosis.

During the last few years numerous investigators have been occupied in the study of the chemical changes which media show during the growth of these bacteria. Papers by Kiessling (36), Péré (34), Blachstein (32), Germano and Maurea (15) and Dunbar (17) all indicate that the differential diagnosis between Eberth's and Escherich's bacteria must be sought for in the chemical changes which the medium undergoes.

It is allowed that some well-defined differences between the typhoid bacillus isolated from the spleen and the bacterium coli commune taken from the bowel do exist, and though Rodet and Gabriel Roux (22) consider that the latter is capable of being transformed into the specific microbe of typhoid fever this view has found more opponents than defenders. The chief points of interest in differential diagnosis may be referred to here.

That gases are developed when Escherich's bacterium is grown upon weak alkaline bouillon to which two per cent. of dextrose or lactose is added was noticed by Th. Smith (23) in 1889. Independently Chantemesse and Widal (24) made

the same observation, and since that time this behaviour has been held as differential, since Eberth's bacillus causes such a medium to become uniformly turbid in twenty-four hours, but after a few days the medium clears and no development of gases takes place. The fermentation of sugar by bacterium coli commune is stated by Dunbar to yield gas after three hours' incubation at 37°C., and by the end of the fourth day an amount of gas is produced which amounts to about one-third of the volume of the culture medium ; this gas consists of carbon dioxide and hydrogen. The new gas-forming bacillus described quite recently by Gärtner (25) from the peritoneal cavity is, according to Klein (26), identical with bacterium coli commune and probably reached this situation by direct migration from the gut. The so-called indol reaction described by Kitasato (27) consists in the addition of 1 ccm. of ·02 solution of potassium nitrite with a few drops of concentrated sulphuric acid to 10 ccm. of a bouillon culture. If this is a pure growth of bacillus typhosus the characteristic red colour seen with cultures of Escherich's bacillus and other typhosus-like bacteria is absent. A reaction described by Zinno (28) and dependent upon the presence of kreatinine is performed by adding a few drops of sodium carbonate and very dilute solution of nitro-prusside of sodium to suspected growths in two per cent. peptone bouillon. An intense red colour develops which gradually fades into yellow. By the addition of acetic acid an emerald green colour appears which passes into blue. These reactions are given by cultures of bacterium coli from many sources but not by bacillus typhosus. Cultures of Koch's spirillum and vibrio Metchnikovi respond to the above tests, which is not the case with the spirillum of Deneke or that of Finkler-Prior.

The behaviour of the typhoid bacillus and allied organisms when cultivated upon sterile milk was first noted by Chantemesse and Widal (29), who established the fact, which has been verified by all subsequent observers, that the bacillus typhosus does not cause coagulation of milk, while this is a constant feature in the growth of bacterium coli commune. In connection with this Eberth's bacillus, though it

will grow upon an acid soil, produces very little lactic acid when cultivated upon sterile whey, while, as Petruschky (30) originally discovered, the bacterium of Escherich produces more than twice the amount of acid. Milk coagulation is apparently due to this development of acid and not to an enzyme, and Huysse (31) has shown that cholera vibrios also possess this property of acid production with consequent curdling of milk.

The researches of Blachstein (32) which were carried out in Nencki's laboratory have added largely to our knowledge of the biology of the bacillus typhosus. The lactic acid produced by the growth of Eberth's bacillus on glucose bouillon is always lævo-rotatory and no ethyl alcohol is produced. The bacterium coli commune grown upon a similar soil produces ethyl alcohol and considerable quantities of dextro-rotatory paralactic acid. Macfadyen (33), while working with Nencki, examined the contents of the small intestine chemically and also for bacteria. These he found especially attacked the carbohydrates within the bowel. Alcohol was formed by all the isolated micro-organisms and the amount produced in some cases amounted to sixteen per cent. of the weight of the sugar. The lactic acids present in the bowel contents were either the especially inactive or the dextro-rotatory paralactic acid. There was apparently no evidence that any other lactic acid was present. Blachstein speaks of three different activities which the bacillus typhosus can exhibit according as this microbe is isolated from dejecta, from the spleen, or taken from subcultures. The first and last of these kinds produce relatively much and little lactic acid, but this is always lævo-rotatory. Calculations have shown that the small intestine of the guinea pig contains about 1400 microbes, and the large intestine 2000-5000 for each decigramme of material. Among this vast number it may be affirmed that no single microbe in the normal gut yields a lævo-rotatory lactic acid, and since Escherich's bacterium is a normal occupant of the gut of infants, men and many animals, it may be confidently affirmed that typhoid fever cannot be spread by normal human dejecta, a view which in this

country, owing to the teaching of Murchison, is still frequently entertained. A transformation of bacterium coli commune into a typical bacillus typhosus is, according to Blachstein, impossible. These allied micro-organisms, which are probably specifically distinct, though the determination of this can only be decided by methods other than morphological, are likely to arouse even greater interest in the future, since it has been shown by Chantemesse and Widal that Escherich's bacterium, though not directly pathogenic for man as it is for some animals, aids greatly in the growth and generalisation of bacillus typhosus in an infected body.

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FOSSIL ALGÆ.

THERE cannot but be general agreement in the view that all theories of the evolution of plant forms must be based on the assumption that Algæ are among the least changed descendants of the earliest forms of life on the globe. They represent an extreme term in the series of autonomous organisms capable of converting inorganic matter into food substance, and though organisms of more simple organisation are known, these are parasitic or saprophytic and accordingly may be, must be, accounted for on the same theory, as derived by degeneration from forms such as Algæ vegetating by their own intrinsic chlorophyll, using the word in its widest sense. Since successive researches of absorbing interest made during recent years have declared an express complexity of cell-organisation even in the simplest forms of Algæ we may further proceed to assume without violence the possibility of the existence even now, or, at all events, in past ages, of still more elementary organisms of like autonomous character representing a more remote ancestral type. However far back we may push such speculative conceptions of elementary organisms it is plain that whether they did or did not exist in past ages, their bodies must have been so little specialised in the direction of stable structures that we cannot hope for their preservation in fossil forms. The student of the primordial forms of life must therefore content himself with fossil Algæ as the representatives of the earliest type, and he is driven to this by another consideration. It is of course conceivable that more primitive organisations than these should occur in fossil form, but from the nature of things we should not be justified in assigning them to the main series of independently vegetating organisms, since, as has been said, we already know simpler forms of degenerate type with which they might be classed with equal propriety so far as the remains of cell-walls, etc., could guide us.

An examination of the testimony of the rocks to the existence in past ages of Algal forms is therefore a process of general interest to biologists and to all who are concerned with the subject of evolution. The most superficial consideration of the matter will result in the recognition of the high degree of improbability that attends the chances of preservation of such delicate structures as the cell-walls of Algæ. Acquainted as we are with the marvels of preservation of minute structure in many plant fossils, exhibiting even cambial cell-walls as in a newly cut section of a living plant, there must yet be borne in mind the rarity of such conditions in geological history, and their limitation, with a few exceptions, to the case of terrestrial vegetation, though aquatic agencies may have operated in its preservation. Prepare ourselves as we may by such antecedent considerations, a revelation of the scantiness of the record comes with an uncomfortable shock, and the object of this article will be attained if the shock act as a stimulus to the undertaking of new searches for material in the older rocks, since the bareness of the record is doubtless due in part to the want of enterprise of this kind. There is a disposition to be content with the reflection that the older the organism the simpler it must be and therefore the less likely to be preserved to us, that the very absence of such forms from fossiliferous strata is an eloquent comment on their character, that there is no use fighting against the malignant conspiracy of natural forces to obliterate the vestiges of early life, but such a disposition is a truant one and inconsistent with the progress of palæontology in general.

By the researches of Nathorst (1 and 2) the ground has been cleared of the debris of many spurious fossil Algæ described by Brongniart and other older writers under the names of *Chordophyceæ*, *Arthrophyceæ*, *Rhixophyceæ*, *Spongiophyceæ*, *Dictyophyceæ*, *Keckia*, *Münsteria*, *Oldhamia*, *Eophyton*, *Discophorites*, *Gyrophyllites*, *Chondrites*, *Confervites*, *Caulerpites*, etc. A considerable number of such forms were thus confidently described and assigned definite places among the Algæ by writers who must have had vague enough notions of the outward forms of the living organisms.

Nathorst pointed out after obtaining similar markings by experiment that many of them were mere trails of animals and other casual impressions, or the remains of other organisms, and while thus sweeping away an encumbrance of little credit to botanical literature he laid down the useful rule that the claims of no organism of the kind should be accepted unless it exhibit actual structure, or, at all events, a rind of coal. That this excellent rule is somewhat too severely exclusive has been pointed out by Graf zu Solms-Laubach (7), since coal "may entirely disappear in the course of time from remains that are undoubtedly organic, if they are deposited in a porous rock". A literal application of it would affect many very definite and characteristic impressions of fossil plants, but there is no gainsaying the healthy nature of his conditions. Any departure from them must be supported by strong evidence and properly safeguarded by a consideration of the nature of the bed as disclosed by other remains. Unfortunately the evidence of impressions of outward forms is little to be trusted in the case of Algæ, since few groups of them exhibit steadfast and characteristic outlines owing to their extreme plasticity in response to their environment. Nathorst's useful results were strongly disputed by the Marquis de Saporta (3), who furnished his own condemnation in the illustrations to his memoir. These examples of astonishing innocence and credulity as to the characteristic forms of Algæ are an eloquent plea for the rigorous employment of Nathorst's conditions. (The reader is referred to Solms-Laubach's *Fossil Botany* (7) and to Zittel's *Handbuch der Palæontologie, II. Abth. Palæophytologie* (8), commenced by Schimper and completed by Schenk, for further details and references to the literature of this portion of the subject.) We are left, then, with a small residuum of organisms, which possess, however, the exceptional interest of being our only known remains of the vegetation of past oceans, their shore Algæ and their plant Plankton.

The great number of Algæ existing at the present day, their variety of form and range of habitat, would lead one to

expect a considerable series of representatives in the fossil-bearing strata. On the other hand, their simple structure and their extremely rapid decomposition form weighty reasons for expecting little, and it will be seen that those Algæ now exhibiting special adaptations of structure to durability are precisely the groups of which representatives are preserved to us. The Diatoms with their siliceous walls, the calcareous *Siphoneæ* and *Corallineæ*, are the Algæ about which one would now be most inclined to prophesy on grounds of structure that their remains are most likely to be embalmed in the deposits now forming in the ocean, and they are all of them in fact found in sediments of contemporary age. If we add the encrusted *Characeæ* as sharing in the chances of preservation no violence will be done to reasonable expectation. It is a very remarkable fact that after the Augean labour of Nathorst and others the only fossil Algæ of any importance left to us belong to these very groups. They are almost the only Algæ of our present seas of which the structure is rendered fairly permanent by mineral encrustation during life. May we not legitimately suppose that in past ages their less resistant companions, organised as they are at the present day, suffered such extinction as it may be presumed they now undergo? If, however, we look more closely into the record our expectations will be enhanced of finding other forms.

The first fossil Alga exhibiting structure and so furnishing adequate claims to recognition appears in the Devonian age, *viz.*, *Nematophycus* of Carruthers, and (with the exception of the doubtful *Pachytheca* of the same age) it stands alone. It was first described under the name of *Prototaxites* by Sir William Dawson, who took it for the wood of a Gymnosperm, but the subsequent examination of it by Mr. Carruthers (4 and 5) dispelled this interpretation and established its claims as an Alga. He placed it among the *Siphonaceæ* (especially *Udoteæ*) and beyond doubt correctly. It is hard to see on what ground Solms-Laubach suggests *Fucaceæ*, since the great uninterrupted lumina of the tubes and the fine lateral haptera or tenacula are wholly inconsistent with the structure of any Fucaceous

plant. It must have formed a noble sea-weed with stems several feet in circumference recalling in stature the Fucaceous and Laminarian giants of our southern seas, and in girth at all events surpassing them. There is no evidence of calcification here, and the presence of tenacula would secure the cohesion of the tubes which are not so much interwoven as those of *Avrainvillea*, for example, which dispenses with both calcification and tenacula. Since *Udotea*, however, which it most resembles, possesses encrustation nearly always slight but varying in amount in different species, in addition to tenacula, it is possible that we may owe the preservation of *Nematophycus* to a slight calcification though no trace of it remain. Disregarding *Pachytheca* as a doubtful case we must leave the primary rocks with this sole veritable representative of the Algæ.

It is difficult to say definitely what is to be made of Bertrand and Renault's *Pila bibractensis* (6) of the Permian epoch. Its authors describe it as a gelatinous Alga with an ellipsoid, multicellular thallus, and appear to see farther into its nature than one is quite prepared to accompany them. It is impossible to assign it a place, and there is in point of fact little to be said and more to be doubted with regard to this remarkable production.

In the great sweeping away of spurious fossil Algæ the Secondary rocks were left quite destitute of any true claimant to recognition until we come to the top of the series in the Cretaceous beds which contain diatoms and *Lithothamnion* (Senonian beds). The forms of *Bactryllium* of Triassic age may eventually be found to survive this denial of their claims as Algæ, and the possibly Siphonaceous *Diplopora* from the Muschelkalk and Lower Keuper, *Munieria* and *Triploporella* from Cretaceous beds, are even more likely to receive adoption into the series of true Algæ. The present writer described (9) a fossil *Caulerpa* (*Siphoneæ*) from the Oolite (Kimmeridge Clay) of which very complete casts in the round are preserved, and it is noteworthy as the remains of an Alga which was not encrusted with any mineral secretion. In these last days Mr. Seward (10), in his admirable account of the Wealden flora given in

his *Catalogue of the Mesozoic Plants* in the British Museum collection, has made a further contribution to the subject. He proposes the generic term *Algites* "for those fossils which in all probability belong to the class Algæ, but which by reason of the absence of reproductive organs, internal structure, or characters of a trustworthy nature in the determination of affinity, cannot be referred with any degree of certainty to a particular recent genus or family". At first sight this appears to be a retrograde movement, but on the whole it is a wise conservatism, and the only danger I can foresee is in the genus being made a convenient limbo for fossils which by no probability belong to the Algæ—new *Eophytons* and *Spirophytons* and the like. Mr. Seward will have to jealously guard a genus of such elastic characters. No one can withhold sympathy from the cautious step he has taken, since it will no doubt often happen in the future that fossil remains indicating Algal nature will come to be recorded by men who would shrink from calling them by generic names that suggest modern affinities like *Caulerpites*, *Chondrites*, etc. It is very much the same position as that adopted early in this century by Dawson, Turner, Robert Brown and others in the study of the living forms. They retained the name *Fucus* for hosts of Algæ which they knew to be far other than congeneric until a proper system of classification could be established. Mr. Seward describes *Algites Valdensis*, an Alga with the dichotomous habit of *Chondrus crispus*, *Dictyota*, *Nitophyllum*, etc., among living forms and *Algites catenelloides*, the specific name of which describes its resemblance to a well-known Floridean form. Mr. Seward also describes the oogonia of a *Chara*, *viz.*, *Ch. Knowltoni Sew.*, and he reminds us of another *Ch. Jaccardi Heer* recorded from the Wealden and another species of Saporta's from the Jurassic system. It is highly probable, however, that these forms ought properly to join the assemblage of problematic Charas that extend back to the Lower Devonian Carboniferous sandstones. Another *Chara* of Secondary age has recently been described by Knowlton (as noted by Mr. Seward) from the Upper Cretaceous Bear River formation of North America. Mr. Seward's own

Chara strikes me as much the best claimant until we come to the Tertiary *Characeæ.*

In passing one ought to note the possible Alga of the Oolitic granules at present attracting the attention of many geologists and resembling the *Girvanella* of Nicholson and Etheridge of Ordovician age, and as Mr. Bullen Newton points out to me the *Siphonema* of Bornemann which goes as far back as the Cambrian rocks. The best specimens reveal no more than a tubular structure consistent with an organic origin, and however strong the temptation may be to regard them as Algal such recognition must be delayed until better evidence is forthcoming.

Castracane has stated that he found several species of diatoms in the ash of English coal and that these are fresh-water forms at present existing. So exhaustive a search has been made, however, by Williamson and other observers that in spite of the fact that Castracane claims to have used due precautions we must treat his record as open to doubt until it is confirmed by further discovery. No diatom appears with this possible exception until we reach the Upper Cretaceous beds, and then they occur again in extensive deposits of Tertiary and Quaternary age. All these fossil diatoms, and they are very numerous, from the chalk downwards, belong to genera and a large number of them to species now existing, though the proportion of identical species diminishes with the age of the deposit. The forms known as *Bactryllium* from the Trias may have been ancestors of the diatoms—it is very doubtful—and but for this one possibility we have no hint of the coming of this type until we find it in a profusion of forms some of which have survived from the Cretaceous age to the present day. One would expect them in the Silurian rocks, but here too there is a blank. Careful search has been made for them by several observers, but the matter is eminently worthy of prosecution and may be urged upon the characteristically industrious diatomist as a more worthy occupation than practical experiments in the origin of species.

Just as at present the diatoms are engaged in making deep-sea deposits in the colder regions of the northern and

southern oceans, the Rhabdospheres and Coccospheres, which we must regard as pelagic Algæ, are playing the same *rôle* in the tropical and temperate seas. The broken-down parts of these organisms known as Rhabdoliths and Coccoliths are found in the globigerina oozes, and they have practically the same geological history as the diatoms. The problem presented by these remarkable organisms is undoubtedly of palæontological interest, but it is primarily a biological one since so little is known of the minute structure and mode of life of the contemporary forms.

Though *Lithothamnion* appears first in the Senonian (Cretaceous) beds it may eventually prove to be as old as the Muschelkalk (Trias). It is not, however, until we reach the Tertiary rocks that this coralline is found occurring massively, as it does in the Lower Eocene. The Leitha limestone (Miocene) and Pisolite limestone and the Nummulitic rocks owe their origin in great part to this contemporary genus. Every botanist who has waded over a coral reef must have been struck by the massive occurrence of this rock-building Alga, the activity of which has been somewhat neglected by students of coral reefs. I think it was Mr. Darwin who remarked that it often formed the cement that bound the coral together. It frequently does more than this. A large number of the specimens brought back by Mr. Bassett Smith, R.N., from his survey of the Macclesfield Bank were *Lithothamnia*, and to witness what this Alga can do in forming a beach in the absence of coral one need not go beyond the British Islands. It is the only Floridean fossil of certain determination unless Mr. Seward's *Algites* should prove to be of this group.

The fossil *Dasycladaceæ* which we owe to the researches of Munier-Chalmas (11 and 12) are of an importance far exceeding all other results in fossil Phycology. With a power of extraordinary divination this author has rescued from among the fossil Foraminifera and elsewhere a series of Tertiary and other Algæ, and having accomplished this remarkable scientific feat has maintained an almost equally remarkable reticence on the subject. In fact we owe to

Graf zu Solms-Laubach and others the better part of the interpretation of the matter. The best defined forms, such as *Polytrypa* from the Grobkalk (Eocene) = the existing *Cymopolia; Uteria* of the same type from the Lower Eocene; *Haploporella* of the type of *Neomeris* and *Zittelina* and *Terquemella* answering to our *Bornetella* of the present day; the more puzzling *Thyrsoporella* and *Prattia, Marginoporella, Dactylopora*, etc., are all Tertiary and chiefly Eocene but occur also in Oligocene and Miocene deposits. Of the same age is *Acicularia*, a genus closely allied to our modern *Acetabularia*, and *Ovulites*, which Munier-Chalmas thinks near *Penicillus* (a Siphonaceous genus outside *Dasycladeæ*) but Solms-Laubach justly determines to be close to *Cympolia*. One may anticipate most hopefully the certain extension of this series backward through Cretaceous, Jurassic and Triassic rocks, and recognition of such forms as *Munieria, Triploporella, Gyroporella* and *Diplopora annulata* (see Gümbel, 13) referred to above, as members of the same series. These forms from the Secondary rocks, however, are fewer and much less definite, but a most fertile field has been opened up here for further investigation, and its fruits are for the cautious investigator, who above all knows his living Algæ.

In the Tertiary rocks we have then representatives of the corallines, *Siphonaceæ, Characeæ* and many diatom deposits, and to these may be added the possible *Cystoseiræ* (*Fucaceæ*) of the Radoboj beds (Oligocene).

The Quaternary rocks continue the history of fossil diatoms and *Characeæ*, and our record closes with Borge's discovery (14) in the glacial clays of the Island of Gotland of a number of Desmids identical with types now alive in the Arctic regions.

The two papers by Mr. James (15 and 16) are mainly of bibliographical interest. He has made a study of the wrecked genus *Fucoides*, its origination by Brongniart, and the numerous additions made to it at various times. The palæontologist will be glad of this service, since the "problematic organisms" in question are often as obscure in

their nomenclature as they are dubious in botanical character.

The most hopeful direction for work then appears to be in the search for further encrusted Algæ, for more corallines, *Udoteæ*, for diatoms in the older rocks and perhaps for other *Florideæ* such as *Squamariaceæ* of which no record has yet been obtained, though the thallus has an equal chance with some of those mentioned. The Cretaceous seas had a plant Plankton like our own in the matter of diatoms and Coccospheres and Rhabdospheres and the older Secondary rocks may yet prove productive of more and better defined forms. The example of Munier-Chalmas is inspiriting, and by following it the great periods laid waste by the successful destructive criticism of Nathorst and others may yet be re-peopled with forms having valid claims to recognition as true fossil Algæ.

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GEORGE MURRAY.

ANCIENT VOLCANIC ROCKS.

THE continental petrologists, with few exceptions, continue to make a fundamental distinction between the "older" (*i.e.*, pre-Tertiary) volcanic rocks and the "younger" (Tertiary and Recent). The hold which this doctrine has maintained in almost every country, but our own, depends upon a conjunction of causes. One of these causes is the almost complete absence in the European area of volcanic activity during Mesozoic times. Owing to this long hiatus, the fresh Tertiary lavas come to be, as a whole, strongly contrasted with their much older Palæozoic equivalents, which in course of time have usually suffered much from the secondary alterations to which volcanic products are peculiarly liable. By regarding the accidental characters thus induced as essential, an impression of wide difference between the older and the newer lavas is fostered.

Another curious fact has contributed to confirm the same idea. Germany, France, and Italy form part of a region within which the Tertiary volcanic rocks belong, in great measure, to somewhat peculiar types. Their relative richness in alkalies frequently gives rise to special minerals and associations of minerals; and the occurrence in other parts of the world of corresponding rocks having a high geological antiquity has been the more easily overlooked since some of these special minerals, such as leucite, are eminently liable to chemical destruction.

The doctrine of "older" and "younger" volcanic rocks, itself a surviving relic of the theories of Werner, has been fortified by these circumstances, and is incorporated in the petrological classifications and nomenclature current in continental countries. The leaders in this branch of geological science have, it is true, seen reason in some instances to modify their views; but it is a singular illustration of the conservative spirit that, even while formally abandoning the idea that the essential characters of igneous rocks depend on their geological age, they still retain it in

their text-books and in their terminology, and consider "Carboniferous trachytes" and "Ordovician rhyolites" unpardonable solecisms.

In this country, although it has contributed far less than Germany to the enlargement of the practical knowledge of igneous rocks, the early and full acceptance of Huttonian doctrines has permitted of somewhat wider views on some questions. Moreover the attention of petrologists has been especially directed to the great groups of Palæozoic volcanic rocks so fully represented in the British Islands, and so, necessarily, to the comparison of them with the Tertiary lavas which fill so large a space in petrological literature. The result has been a revolt against the above-mentioned dogma and a well-founded conviction among English students that the supposed differences between the older and the younger volcanic rocks reduce to the fact that the former are, as a rule, more affected than the latter by the changes which come with lapse of time.

Some part of the difficulty seems to have arisen from confusing volcanic with plutonic rocks. The latter being formed under deep-seated conditions and brought to light only by long-continued erosion, those actually seen belong for the most part to pre-Tertiary times; and indeed the geologists who still cling in some degree to Wernerian ideas have only reluctantly come to admit the existence of granites, gabbros, etc., of Tertiary age. Among the older strata it is not always easy to distinguish, by field-evidence alone, between intruded and contemporaneous igneous rocks. Hence, when it is stated by some writers that certain minerals, such as muscovite, microcline, rutile, tourmaline, and topaz, are found in the older but not in the younger rocks, a more correct form of statement would be that these minerals are characteristic of plutonic rather than volcanic rock-types. In any form the statement is not true without qualification. Hypersthene is another mineral which was once considered to be characteristic of the older rocks, it being known at that time as a rock-constituent only in the large and usually "schillerised" crystals in which it occurs in hypersthenites and gabbros. Whitman Cross in 1883

showed that hypersthene in small crystals of very different appearance is a constituent of widespread occurrence in Tertiary andesites, and it has since been recognised as equally abundant in andesites of Palæozoic age.

The minerals which have been considered peculiar to the Tertiary volcanic rocks are leucite, nosean, haüyne, melilite, and tridymite, and some of these are still scarcely, if at all, known in the older lavas. Considering, however, the rarity of melilite-basalts and haüyne-bearing rocks among the Tertiary and Recent lavas, it is not a matter for surprise that they should remain unknown among lavas which have received less attention.

What is perhaps more significant is the fact that all these minerals are peculiarly liable to decomposition, and of some of them, at least, all trace would easily be obliterated by secondary changes in the rock. The case of leucite is especially interesting, for here, although the mineral is commonly destroyed in the older rocks, its unmistakable crystal-form is in some instances clearly preserved by its decomposition products. Of this nature seem to be the "pseudo-crystals" composed of orthoclase and nepheline described by Hussak and Derby in the phonolite and foyaite of Tingua Mountain in Brazil. Opinion was somewhat divided as to the nature of these pseudo-crystals, but doubts may be considered to be set at rest by the study of similar pseudomorphs in other districts, and especially by J. F. Williams' description of the leucite-syenite of Magnet Cove in Arkansas. If the large leucite-crystals in such rocks have become obscured, it is easy to understand how the small crystals of volcanic rocks may have been altogether obliterated.

Palæozoic leucite-lavas, however, are not wholly unknown, and one has been described by Von Chrustschoff (1) from Siberia. It occurs on the right bank of the Tunguska, forming a flow distinctly overlain by limestones containing *Favosites*, *Halysites*, *Cyathophyllum*, and other characteristic Lower Palæozoic fossils. Little white crystals of leucite, up to 1 mm. diameter, are visible in a compact groundmass, and the microscope shows augite, anorthoclase, sani-

dine, nepheline, and accessory constituents, with some residual glassy base.

As regards nepheline, this mineral, like some others, is known in small perfect crystals and in larger and more shapeless ones with numerous inclusions (elæolite), and these two varieties are now recognised as characterising, not "younger" and "older" rocks, but volcanic and plutonic rocks, respectively.

Again, it has been alleged that the sanidine variety of orthoclase and the microtine varieties of the plagioclase felspars are restricted to Tertiary and Recent volcanic rocks. Sanidine is not very strictly defined, and the name is often used to imply merely a fresh glassy appearance, the loss of which in the felspars of the older lavas is easily explained by incipient chemical decomposition. In some instances, however, undoubted Palæozoic rocks contain felspar showing the characteristic crystal-habit, the orthopinacoidal cleavage, and the glassy lustre of typical sanidine, and the distinction is clearly one upon which no stress can be laid.

It is, of course, well known that many of the Tertiary lavas contain a considerable amount of isotropic glassy base, and some of them (obsidians) consist almost wholly of glass. The supposed absence of these glassy types among the older formations was pointed out by those who maintain an essential distinction between the pre-Tertiary and the Tertiary lavas. The answer was given by Allport, to whom in the first place belongs the honour of clearly seeing and upholding the essential identity of corresponding volcanic rocks of all ages. In the group of volcanic rocks near Wellington in Shropshire, now known to be of pre-Cambrian age, he pointed out (1877) all the characteristic structures of the fresh Tertiary rhyolites of Hungary, etc., still evident in rocks which have lost their glassy nature by molecular changes. These ancient obsidians and rhyolites show, in different examples, trichites, perlitic cracks, spherulites, etc., and they are accompanied by rocks which are recognised as altered volcanic ashes. Rutley and others have shown that similar *devitrified* acid lavas have a very wide distribution in the Ordovician of North Wales,

Westmorland and other districts, and the work of Bonney and Cole makes it appear that even such special structures as the curious lithophysal cavities in some fresh obsidians find their analogues among these ancient rocks.

Allport had already shown (1870, 1874) that the basic rocks—basalts and dolerites—of the British Isles, of late Palæozoic age, are, except for secondary changes, mineralogically and structurally identical with the late Tertiary rocks of similar composition in Europe. Other English authors have described our older intermediate lavas with the same general result, Teall's study of the Cheviot hypersthene-andesites, of Old Red Sandstone age, being especially noteworthy (1883). Most of these rocks are considerably altered, and have been known in the field under the name "porphyrite," a term largely applied to altered andesites in other districts; but one rock, the "pitchstone-porphyrite" of some authors, is relatively fresh, with partially glassy base, and closely resembles the Tertiary andesites of Hungary and the lavas erupted from Santorin during the present century. A closely allied type has since been recognised in the Ordovician of Carnarvonshire.

Some of the most interesting of recent contributions to our knowledge of ancient volcanic rocks come from the Carboniferous districts of Scotland and Ireland. In the earlier part of the Carboniferous period the area which is now Southern Scotland experienced a great outpouring of volcanic material. The discharge of lavas was so copious as to give rise now to broad table-lands and ranges of hills, sometimes many hundreds of square miles in extent, and Sir A. Geikie (2) distinguishes this phase of vulcanicity as that of the "plateau" eruptions, contrasting these with the more restricted and sporadic "puy" eruptions of a somewhat later time in the same area. He enumerates five plateaux, marking as many independent centres of volcanic activity, the original extent of the largest being estimated at between 2000 and 3000 square miles. The materials representing these great eruptions are mainly successions of lava-flows, but with subordinate beds of tuff. The greater part of the lavas have not yet been closely studied, and are designated

by the general field-term "porphyrite". Judging from the variety of rock-types recognised by the examination of the small plateau of the Garlton Hills in East Lothian, it seems probable, however, that the whole group will be found to yield many results of interest.

Some of these Garlton Hills rocks have been described by Hatch (3). They are partly from lava-flows, partly from "necks" which are believed to mark the actual sites of volcanic vents. In some cases the rocks have retained their original characters with remarkable freshness. The lower lavas are of thoroughly basic composition, and, for the most part, are olivine-basalts with from 46 to 49½ per cent. of silica. One, however, is of ultrabasic nature, with only 40 per cent. of silica and large proportions of lime, magnesia, and ferrous oxide. Microscopical examination shows it to be a well-marked type of limburgite or magma-basalt, comparable with those of the Kaisertuhl and other districts. Felspar is unrepresented, except by an occasional skeleton-crystal. The olivine is often quite fresh. The augite is a titaniferous variety, showing the characteristic violet tint and pleochroism. The crystals of these minerals, with some magnetite, lie in a ground-mass consisting of augite-microlites and glassy matter, and some nepheline is probably present.

The upper lavas are equally interesting, being chiefly unaltered trachytes with 58½ to 62½ per cent. of silica and 10 per cent. of alkalies. They are holocrystalline rocks, sometimes markedly porphyritic, and consist essentially of fresh felspars with a small proportion of a green soda-bearing augite or ægirine. They are indistinguishable from many Tertiary ægirine-trachytes. Some of the volcanic necks in the district, such as North Berwick Law and the Bass Rock, are formed of trachytes not materially different from those of the flows; but one of them, Traprain Law, shows an interesting difference. It consists mainly of little sanidine-prisms with crystals of a bright green soda-augite, but there are also little patches which close examination proves to consist of nepheline and its alteration-products. The rock is therefore a phonolite, though of a type

poor in nepheline and approaching the trachytes in character.

It is to be expected that further examination will discover lavas of these various types in other parts of Britain. Watts has already noted in the Limerick district a limburgite lava, also of Carboniferous age, and Hobson (4) has described from a neighbouring locality the allied type augitite, in which olivine as well as felspar is wanting, the rock consisting essentially of two generations of augite and magnetite with some base which has probably been glassy.

Meanwhile individual geologists in Germany and other countries have recognised the importance of secondary changes, such as devitrification, in discussing the characters of the older volcanic rocks, and much light may be expected to be thrown upon the various "porphyries," "ceratophyres," "porphyrites," "melaphyres," "schaalsteins," etc., when they come to be compared, from this point of view, with recent lavas. Some of the researches of this kind already published have a special interest as referring to what may be regarded as classical districts for geologists.

In this connection we may profitably notice Sauer's (5) work on the pitchstones and porphyrites of the Meissen district in Saxony. These form respectively the lower and upper parts of a series of lavas, both underlain and overlain by tuffs, and Sauer's survey conclusively establishes the true volcanic nature of the whole. The data are wanting to fix their precise age, but they almost certainly belong to a late epoch in the Palæozoic. The well-known "pitchstones" are acid rocks containing only scattered crystals in a ground essentially of glass. Minute granules of black iron-ore occur in the glass, sometimes aggregated in rows. Large spherulites are met with in one type. Perlitic cracks are universally present. A feature which several observers have noted is the frequent occurrence in the glassy ground of irregular, cloudy or "microfelsitic" patches. Sauer points out the relation of these patches to the perlitic cracks, a clear evidence of their secondary origin, and regards them as due to the devitrification of original glass. By the spreading of this change, the whole of the glassy ground

becomes replaced by the "microfelsitic" substance, the original perlitic structure being sometimes still traceable, like the veins in the mesh-structure of serpentine derived from olivine. More interesting is the tracing of a passage from this type into the "Dobritz porphyry," which had formerly been regarded as a quite distinct rock. In the microcrystalline ground of the latter rock occasional relics of the perlitic structure are still preserved, as well as the microlites of iron-ore, now converted to limonite, and in places spherulites. The evidence of conversion of a glassy to a cryptocrystalline and finally to an evidently micro-crystalline rock seems to be complete at every point.

An interesting question raised is that of the relation of devitrification to dehydration. The author considers that in the first stage of devitrification of the pitchstone there has been, not a loss, but a certain gain of water. In the passage to the microcrystalline state, however, the rock has lost most of its water, and the consequent contraction has given rise to crevices and hollows.

The so-called porphyrites of the district vary from what is described as a normal mica-porphyrite to one comparatively rich in quartz. They are almost all greatly decomposed; but one, termed porphyrite-pitchstone, is relatively fresh, with an unaltered glassy base. It contains enstatite, as well as plagioclase, quartz, hornblende, and biotite, and if it occurred in Hungary would doubtless be named enstatite-dacite. The author gives reasons for believing that this rock represents the original type of the whole group, the microcrystalline ground-mass of the majority being a result of devitrification. His researches therefore go to establish that not only may quartz-porphyries (in the descriptive sense) be derived from glassy rhyolites, but also quartz-porphyrites from glassy dacites or quartz-andesites.

The most complete account yet given of a limited group of ancient volcanic rocks is perhaps Mügge's (6) description of the so-called "Lenneporphyre". These rocks, occurring in the Middle Devonian of the Lenne district in Westphalia, have been the subject of much discussion. Mügge shows conclusively that they are contemporaneous igneous rocks,

in part lavas, in part fragmental accumulations. The lavas are distinctly acid rocks with a general preponderance of soda over potash, and may thus be described as rhyolites or soda-rhyolites. Following the German terminology, the author styles them *Keratophyre.* They are divided into quartz-ceratophyres and felsite-ceratophyres, according to the presence or absence of porphyritic quartz, although the latter rocks are scarcely less acid than the former. One type has abundant porphyritic crystals of corroded quartz and alkali-felspars with flakes of pale mica, which is not muscovite but a bleached biotite. There are sometimes large nodular bodies (giant spherulites) with a central hollow, and there may be relics of a spherulitic structure on a smaller scale, but the ground-mass is believed to have been largely glassy. It has now completely lost its original characters, and in some cases consists mainly of quartz granules. There has evidently been not mere devitrification, but an introduction of silica, which is proved by the high silica-percentage of the rock, reaching as much as $82\frac{1}{2}$. There are little veins of opal in the ground, as well as quartz. From this massive rock every gradation is seen to a schistose and very sericitic kind of the so-called "porphyroid," which here, as elsewhere, is proved to result from crushing.

Another type is much poorer in porphyritic crystals. The ground-mass, though containing veins and nests of secondary quartz, is richer in orthoclase than the preceding, and usually shows little altered spherulites.

The felsite-ceratophyres lack the quartz and mica of the former types. Soda preponderates greatly over potash in the analyses, and the silica-percentage is about 79. All these rocks show a strongly marked flow-structure, which passes uninterruptedly through certain large skeleton-spherulites. The latter (closely paralleled in some of the Welsh Ordovician rhyolites) sometimes make up almost the whole bulk of the rock. These rocks too are subject to crushing, and, as in the former types, the schistose varieties have not the high silica-percentage of the uncrushed.

Mügge makes a minute study of the fragmental rocks associated with the lavas. Some of them are pure tuffs consisting largely of ash-particles, which often show the characteristic concave outlines suggestive of comminuted pumice. In others, which the author styles "tuffites," the ashy particles are mixed with ordinary sedimentary material. The paper, illustrated by seven plates of photographic figures, is a valuable addition to our knowledge of the older lavas.

The Swedish geologists have in recent years described various ancient lavas in every way comparable with the rhyolites, etc., of Tertiary volcanic districts. Such, according to O. Nordenskjöld (7), are the "hälleflintas" which form part of the Archæan formations in the south-east of Sweden. He separates them from the ore-bearing sedimentary "hälleflintas" of Central Sweden, and shows that many of them have "characters in which they agree with the ancient English felsitic rocks, which have been described by the authors of that country as, in part, rhyolites and devitrified obsidians". The correspondence is indeed very close, and is only occasionally obscured by a certain schistosity due to crushing. The rocks are usually porphyritic with crystals of plagioclase, orthoclase, and less frequently quartz. The ground-mass varies from cryptocrystalline to somewhat coarsely crystalline, and frequently shows fluxion and banding, eutaxitic structure, microspherulitic portions, and other characteristic features of the acid lavas. In some of the rocks a cryptocrystalline ground-mass shows regular perlitic cracks occupied by secondary minerals, besides trichites, margarites, and grouped crystallites: these are obviously devitrified obsidians. Very interesting are the so-called "conglomeratic hälleflintas". The supposed pebbles are found to be really altered large spherulites with concentric shell-structure and central space occupied by calcite, quartz, etc., and the description corresponds identically with those of the "nodular felsites" or altered coarsely spherulitic rhyolites so well known in North Wales.

In Central and Littoral Sweden and across the Gulf of Bothnia, in Finland, Högbom (8) has recorded volcanic

rocks belonging to several distinct types. They are post-Archæan but apparently pre-Cambrian, and seem to be in part superficial lavas, in part volcanic dykes. They include syenite-porphyry (a porphyritic trachyte), augite-porphyrites (augite- and hypersthene-andesites), melaphyre (olivine-basalt), and spilite (amygdaloidal andesite). Judging from the descriptions and photographic figures given, these rocks show only the ordinary changes, such as the conversion of olivine to serpentine and hypersthene to bastite and the filling of the vesicles by secondary products, all essential characters being beautifully preserved.

We may mention in passing the remarkable occurrence of acid volcanic rocks, in a perfectly fresh condition, on the shores and islands of Lake Mien in the south of Sweden, where they occur surrounded by gneisses, granite, diorite, etc. These rocks were discovered as blocks and *in situ* by Holst (9), and have been microscopically described by Szádeczky (10). They include glassy and microspherulitic rhyolites, rhyolitic tuffs and breccias, etc., and have not suffered devitrification. In this case, however, there seems to be no clue to the age of the rocks, and the eruption is probably to be referred to no very distant epoch.

Our knowledge of the older volcanic rocks is perhaps less complete as regards the basic than as regards the acid types, but Barrois (11) has given a valuable description of a series of basic eruptions of Lower Palæozoic age in Brittany. The rocks, which we should term dolerites and augite-andesites, with associated tuffs, are developed in the Menez-Hom district in the department of Finistère, and belong to several horizons in the Ordovician and Silurian systems. The massive rocks occur partly as dykes and intruded sills but mainly as undoubted *coulées*, and are described as various types of diabases and augite-porphyrites. Of these the former belong to the earlier eruptions, which were submarine, while the latter are found especially characterising the later eruptions, which were subaerial, the differences between the two sets of rocks being due to the different conditions under which they consolidated. It is also noticed that the diabases occur only in thick flows,

and graduate at the edge into andesitic types ("porphyrites," etc.), which also form the smaller flows. The diabases or dolerites have sometimes a granular, sometimes an ophitic structure, and only very exceptionally contain olivine. The felspar is sometimes labradorite, sometimes andesine, and in the latter case micropegmatite is also present. The augitic porphyrites show every gradation from holocrystalline to perfectly glassy rocks, and this in two parallel series, one of ordinary andesitic types, the other with a variety of radial, spherulitic and variolitic structures. The thoroughly glassy type, which in at least one flow retains its original character, is of great interest from the extreme rarity of such rocks, except as narrow selvages. The basic glass, often with scoriaceous and pumiceous structures, is also an important constituent in the fragmental volcanic accumulations associated with the lavas. These occur abundantly except in connection with the earliest outbreaks, in which the volcanic activity had not yet reached the explosive stage. There are subaerial tuffs, composed of more or less fine volcanic *débris*, enclosing spheroidal scoriaceous bombs of andesitic material and other fragments, and cemented by chlorite and other secondary products. Distinguished from these are stratified submarine tuffs, sometimes fossiliferous, or again containing pisolitic iron-ores not unlike those found in Antrim in similar circumstances. There are also beds of breccia, consisting of angular fragments of the porphyrites cemented by calcareous matter. Barrois clearly shows in this memoir how in the Menez-Hom district erosion has laid bare almost the whole apparatus of vulcanicity in those early times. The phenomena are in many respects comparable with those of the Carboniferous in the basin of the Firth of Forth as described by Sir A. Geikie (1879), except that "necks" marking the actual orifices of eruption have not yet been discovered in the Finistère region.

In America the recognition of the essential nature of the older volcanic rocks has been retarded by various circumstances. An immense development of such rocks, chiefly of pre-Cambrian age, exists along the eastern border of

North America and in other areas on that continent; but they have been, for the most part, so modified by dynamic and other metamorphosing agents, that superficially they differ widely as a whole from the fresh Tertiary lavas so magnificently displayed to the west of the Rocky Mountains. Their significance has thus been overlooked both by Wernerians, such as Sterry Hunt, and by the holders of certain extreme "metamorphic" theories, such as Dana and Logan, the one school regarding the schistose and foliated crystalline rocks as something *sui generis*, the other considering them as necessarily altered sediments. To this it must be added that petrology in America has drawn its inspiration largely from Germany, and not a few of the younger workers have been trained partly in the laboratories of Leipzig, Heidelberg, etc.

Nevertheless a number of American geologists, Selwyn, Wadsworth, R. D. Irving, G. H. Williams, Van Hise and others, have clearly recognised these ancient volcanic rocks as lavas and tuffs of various petrographical types, whose differences from corresponding products of Tertiary and Recent age result merely from the vicissitudes through which they have passed in their long life-history; and the description of the abundant material from this point of view has already been begun. The ancient volcanic rocks in America about which we have at present most information are situated in the country about Lake Superior and in what we may call, in a broad sense, the Appalachian region, occupying an enormous extent in the east of the United States and Canada.

In the Lake Superior region it has been recognised, chiefly owing to the work of Wadsworth, Pumpelly, Irving, Williams, and Van Hise, that volcanic rocks of various types play an important part in the constitution of some of the extensive pre-Cambrian formations there developed. Some of these rocks have undergone more or less profound alterations, and in the detailed study of them much still remains to be done.

As regards the eastern region, Diller gave in 1881 a somewhat detailed account of the petrographical characters

of the felsites and associated rocks of Marble Head in Massachusetts, which Wadsworth had recognised two years earlier as devitrified rhyolites and altered ashes ("porodites"). More recently some advance has been made in the study of the old volcanic formations of other tracts.

To G. H. Williams (12) we owe a clear account of some of the ancient lavas of the Appalachian region as developed at South Mountain in Pennsylvania and Maryland. The majority of the rocks are old rhyolites, and, in spite of secondary changes and frequent crushing, the author recognises not only the flow and banded structures, but spherulites, lithophyses, pumiceous modifications, and other well-known features of acid lavas. The rocks carry porphyritic quartz and an alkali-felspar, but rarely any ferromagnesian mineral. The ground-mass is a mosaic of quartz and felspar, but that this is often due to devitrification and recrystallisation is proved by the preservation in many instances of the characteristic structures of the acid glasses. Basalts are also found, retaining their original structures, ophitic, etc., and skeleton-crystals of olivine, but usually more weathered and more crushed than the rhyolites. In both types of lava epidote is one of the dominant secondary minerals, and in the rhyolites it is in great part the manganese-epidote, piedmontite. The lavas are accompanied by fine banded ashes, flow- and tuff-breccias, pumiceous bombs, and other concomitants of ordinary volcanic eruptions. The whole group underlies strata containing a Lower Cambrian (*Olenellus*) fauna.

In a later paper Williams (13) extends his observations, and describes typical devitrified obsidians, rhyolites, breccias, etc., from Maine in the north and from North Carolina in the south. From a general review of the literature of the subject, he shows the high probability that such ancient lavas and pyroclastic rocks will be found to occur throughout the whole region from Eastern Canada and Newfoundland to the Carolinas and Georgia. In numerous cases, although no detailed descriptions have been given, geologists have recognised the igneous, and sometimes the volcanic, nature of the rocks. In other cases there is reason to believe that

such terms as "metamorphosed slates," "siliceous slates," "ribbon jasper," "chert," etc., have been applied by field-geologists to wide extents of old volcanic rocks. Many of these are of pre-Cambrian age (Huronian, etc.), but others occur at various horizons among the Palæozoic strata. From so large a field it is to be anticipated that valuable information will be forthcoming in the near future.

Researches in various parts of America have also done something towards filling the gap in point of time between Palæozoic and Tertiary igneous rocks. Indeed even in Europe this gap seems to have been somewhat exaggerated. Both intrusive and volcanic rocks of Triassic age are known in the Tyrol, in the French Alps, etc. Others of Cretaceous age occur in Portugal, in Moravia, Silesia, Galicia, and the Russian province Wolhynia, and in the Crimea. Some of these are of peculiar characters; others have been described by Lagorio under such names as mesodacite, mesobasalt, etc., and differ in no essential from their younger equivalents. According to Reyer and others, forerunners of the Tertiary trachytic eruptions appeared in the Euganean area as far back as the Upper Jurassic period. Such facts have, of course, been appreciated by many continental geologists, and when such influential leaders as Rosenbusch consent to abandon the age-criterion in the classification of rocks and revise their petrographical nomenclature accordingly, a serious obstacle to the progress of petrology will have been removed.

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Alfred Harker.

THE MEASUREMENT OF TEMPERATURE.

IN these days of specialisation the methods of investigators differ as widely as their aims, and apparently they have little in common except the desire to do their utmost for the advancement of science. Let us suppose an unscientific but intelligent visitor to pass, say, from a physical to a chemical, and thence to a biological laboratory. Except that in each are many instruments built up of glass and metal their contents would present but little similarity, nor, with one exception, would repetitions of the same apparatus be noticeable. One instrument, however, would be common to all, would be found in every room, possibly on every table, and inquiry as to its name would, I need scarcely say, receive the reply : " A mercury thermometer ". Our visitor would be led to the natural conclusion that such an essential instrument to which so much attention must have been devoted would have arrived at a high degree of perfection and that its history would be one of progress and evolution.

Such being the case it is strange to reflect that the mercury thermometer has remained practically unchanged from the time (1714) when Fahrenheit first suggested the use of certain fixed points to the present day.[1] True, Cavendish in 1780 investigated the conditions under which observations with this instrument must be made, such as the effect of the immersion of the stem, etc.,[2] and in recent times the labours of Regnault, Rowland, Crafts, Pickering and Guillaume have added much to our knowledge of its imperfections ; although, with the exception of the last named, they have done but little to help us to remedy them.

[1] The origin of the Fahrenheit fixed points is so little known that I may be excused a reference to it. His lowest "natural cold" was that of his freezing mixture, his "highest natural heat" that of the human body. Thus the freezing and boiling points of water are, on that scale, consequent, not original ones (see Gamgee, *Cam. Phil. Soc. Proc.*).

[2] Thorpe, *Essays on Historical Chemistry*, p. 75.

SECTION I.

Errors arising from faulty determination of temperature are, I believe, much more common than is usually supposed to be the case, and in an earlier number of this review[1] I have called attention to some of the lamentable consequences arising from such errors. True, the labour involved in the complete standardisation of a mercury thermometer is appalling. In addition to the necessary observations of the accuracy of its "fixed points" it includes :—

(1) A calibration of the bore.

(2) The determination of the temporary changes of zero and repeated observation of its permanent rise.

(3) The estimation of differences caused by movements from the horizontal to the vertical position.

(4) Observation of the effect of changes in the external pressure.

(5) Determination of the differences resulting from the rate of rise of temperature.[2]

(6) The application of the correction (at best but approximate) for any unimmersed portion of the stem.

(7) The reduction from the mercury in glass scale to the air scale.

No man whose time is valuable can, as a preliminary to some investigation, afford to spend months in the determination of such quantities, or, having done so, contemplate unmoved the fact that a careless motion of the hand may render all his labour of no avail. Let us suppose, however, that all the above quantities have been successfully ascertained, the labours of the observer are by no means ended, for one determination of temperature is of itself a lengthy operation. I can best illustrate this by an extract from my note-book, of the observations and corrections in a single case (Tonnelot's hard glass thermometer, No. 11,048).

[1] April, 1894.

[2] No accurate determination can be made by a mercury thermometer whose temperature is falling (see *Phil. Trans.*, 184 A, p. 442).

OBSERVATIONS.

A	Reading (stem vertical and all at one temp.)	25·615°
	Barometer (corrected)	754·8 m.m.
	Distance from centre of bulb to surface of water	400 m m.

The thermometer was then placed in melting ice, and watched until the *lowest* point touched by the column was ascertained.

B	Reading (stem vertical)	0·051°
	Barometer (corrected)	754·8 m.m.
	Distance from centre of bulb to surface of water	66 m.m.

CORRECTIONS.

A	External pressure = $754{\cdot}8 + \frac{400}{13{\cdot}6}$ . . .	= 784·2 m.m.
	Hence external pressure correction .	= – 0·003°
	Internal pressure correction . . .	= + 0·041°
	Calibration correction	= + 0·004°
	sum	= + 0·042°
B	External pressure = $754{\cdot}8 + \frac{66}{13{\cdot}6}$ . . .	= 759·7 m.m.
	Hence external pressure correction .	= 0·000°
	Observed zero depression . . .	= – 0·051°
	Calibration correction	= 0·000°
	Internal pressure correction . . .	= + 0·008°
	Hence true zero depression	= – 0·043°

Hence sum of corrections from A and B	= + 0·085
Hence corrected reading = 25·615° + 0·085	= 25·700°
Fundamental error	= – 0·011°
∴ Temperature on true mercury scale	= 25·689°
Correction to hydrogen scale . . .	= – 0·096°
True temperature on hydrogen scale .	= 25·593°

Had the temperature not been steady, a further correction depending on the rate of rise would have had to be included.

Although in the above example many of the corrections are of opposite signs and the total effect small, this is not

necessarily the case. Occasionally all the signs are similar, and the effect is considerable, amounting in some cases that I have observed to nearly 0·4°C. Again, the corrections in the case of hard glass thermometers are smaller than when soft glass is used, and nearly all the thermometers used in our English laboratories are of the latter material.

It is therefore obvious that the endeavour to obtain one accurate reading by means of a mercury thermometer involves considerable labour, and presupposes a prolonged and exhaustive study of the thermometer used. Many observers disregard these precautions and defend themselves by the following statements: (1) that certain of these corrections are uncertain and possibly imaginary; (2) that cases requiring such extreme accuracy are rare. The opinion of such an authority as Dr. Guillaume ought alone to be a sufficient reply to No. 1. It is, however, an objection which I confess to having at one time entertained, and I find it is very prevalent. The best answer that I can make is to give particulars of a case which has come under my notice within the last few weeks.

In February, 1893, I received from the International Bureau of Weights and Measures the thermometer by Tonnelot, No. 11,048, and in *Phil. Trans.*, A, vol. clxxxiv., pp. 427-433, I have already published a full account of the comparison of this thermometer with my own standards. In the autumn of last year I received from Professor Threlfall of Sydney University a request to procure for him, if possible, thermometers fulfilling certain specified conditions. Three thermometers were accordingly prepared by Tonnelot, standardised at the International Bureau and they reached my hands the first week of June, 1894. One of these was for low temperature, and I have made no comparisons between it and others. No. 11,402 had a range from − 1° to + 21°C. No. 11,403 had a range from − 1° to + 1° then a bulb and then from 20° to 41°C. Each degree on these thermometers was divided into twenty parts.

On June 8th and 9th Mr. C. T. Heycock was so good as to assist me in a careful comparison of these instruments

with No. 11,048 and with some other thermometers. I had but one suitable micrometer scale, and this was used when observing No. 11,048, which was divided to $\frac{1}{10}$th of a degree. The probable limit of observational error in this case was therefore only 0·001°C. The other thermometers were observed through ordinary reading telescopes, and as it was impossible to estimate fractions of the spaces less than $\frac{1}{10}$, the probable limit of error of a single observation was 0·005°C. The actual *uncorrected* readings differed by as much as 0·4°C. in certain cases. The *corrected* results of the separate readings differed in no case by more than 0·010°C. and only in one case by more than 0·006°C., which is about the probable observational error. The mean result of the whole of the thirteen comparisons made between 11,402 and my standard (taken at regular intervals from 12° to 21°C.) gives an absolutely identical result of 17·1417!

Only four comparisons were made between 11,403 and my standard, the means being 25·197 and 25·200 respectively. The exact correspondence in the former case must be in part fortuitous, but results such as those are a wonderful testimony to the accuracy of the tables of corrections supplied by the Bureau, and an examination of the full table of numbers obtained by us during the above comparisons would, I think, convince the most sceptical of the validity and necessity of the various corrections.

As regards objection No. 2, if accuracy of such an order is possible there can be no doubt of its desirability. For example, the perfection of our methods of electrical measurements is now so great that errors amounting to 0·01°C. in the estimation of the temperature of Clark cells and of resistance coils, etc., become serious, and all efforts to trace, by means of a mercury thermometer, the variation in such quantities as the specific heat of water and other substances with change of temperature, are hopeless, unless the previous standardisation of the thermometer has been satisfactorily accomplished.

I do not wish in any way to disparage the standardisations performed at Kew, but it is evident that they do not

meet, and I conclude are not intended to meet, the demands of exact inquirers. Very wisely they, in no case, give the correction term beyond 0·01°C., for they do not supply any of the data (as far as my knowledge goes) for making the corrections rendered necessary by change in position, in external pressure, in the temporary change of zero consequent on the preceding temperature of the thermometer, etc., and as the combined effect of such corrections alone may considerably exceed 0·01°C. it is evident that until the department supplies further information it has carried its method of correction to its limits. I am afraid that the issuing of those Kew certificates, which give the second decimal figure of the correction, so far from being a prevention is a cause of inaccuracy, as, unless used with a full knowledge of the variations consequent on changes in the conditions, they impart a false confidence to their possessors.

In this section I have endeavoured to show that although it is *possible* to accurately determine ordinary temperatures by means of a mercury thermometer, the process is an intricate and unsatisfactory one at the best, and that even under the most favourable circumstances, the determination of a *single* temperature requires considerable expenditure of time and attention. The removal of the thermometer from its initial position, its immediate immersion in ice, the complete immersion of the stem, the measurement of the external pressure, etc., make up a series of operations which the conditions of an experiment usually render difficult, if not impossible.

SECTION II.

A consideration of the difficulties inseparable from the use of mercury thermometers would naturally lead to the conclusion that if some more simple but accurate form of thermometer, free from such unavoidable causes of error, could be devised, it would meet with general acceptance. The platinum thermometer, as now constructed, is an almost ideal instrument, and is especially free from those defects inseparable from mercury thermometers. It can be used under circumstances which forbid the employment of a mercury one, and it surpasses that instrument both in accuracy and in

range. It is difficult to account for the fact that such an instrument remains comparatively neglected, and I am glad of this opportunity of bringing its claims under the notice of so large a number of scientific workers. I propose to briefly review the evidence we now possess concerning the accuracy of this method of determining temperatures.

The bibliography of this subject is small, and at the end of this article will be found what I believe is a fairly complete table of such communications as have been published. I shall refer to these papers by the figures allotted to them in that list.

I do not propose to enter into any details as to the method of making or using these instruments as full information on such points will be found in papers (7), (8) and (9); for my object is rather to present the evidence in favour of their accuracy in such a manner that its cumulative weight may be appreciated. It may be advisable to first clear the ground by disposing of some objections which I find are commonly entertained by (I may remark) those who have had no experience in the use of the instrument.

1. The general scepticism as regards the constancy of the platinum thermometer may, I think, be traced to the adverse report delivered by the influential committee appointed by the British Association in 1873 (1). In 1874 they reported that the indications of the instruments examined changed by as much as 50°C. after heating in a common furnace, and also that the fixed points altered by mere lapse of time. It must, however, be remembered that this committee was appointed to examine, and did examine, but one form of apparatus, *viz.*, the Siemens pyrometer, and their adverse conclusion should in no way affect the reputation of the modern form of instrument.

In Siemens's pyrometer a platinum wire was wound on common clay, placed in an iron tube, and exposed to a high temperature. Changes exceeding those found by the committee would be anticipated by observers who have studied the behaviour of platinum wires under such circumstances, and my own feeling is one of surprise that the

alterations were so small! If constancy is to be secured the protection from the furnace gases must be complete, and the material on which the wire is wound selected with great care. It is a significant fact that the committee pointed out that the resistance of the coil which was placed in a platinum, instead of iron, sheath remained practically unchanged, and it is probable that what small change was noticed therein was a consequence of the insufficient annealing of the wire. Again, the relation between the resistance and the temperature of the wire does not appear to have been determined by Sir William Siemens with sufficient accuracy. The formula suggested by him, $R = \alpha T^{\frac{1}{2}} + \beta T + \gamma$, is deduced from insufficient data.

I extract the following from Callendar's paper ((9), p. 110): "It appears from Sir William Siemens's account of his experiments that they were undertaken rather with a view of graduating a commercial pyrometer than of investigating the law of change of electrical resistance. Temperatures up to 350°C. were determined *by mercury thermometers* in an air- or oil-bath, and it does not appear that any corrections were applied to their readings. The individual observations are somewhat irregular and often show divergencies amounting to two per cent. and over. Only three observations at higher temperatures are given; they show a mean deviation of about 30°C. A copper ball-pyrometer was used to determine the temperatures, which are given as 810°, 835° and 854°C.; the corresponding temperatures deduced by Siemens's formula from the observed resistances of the platinum pyrometer were 772°, 811° and 882°C. . . . The resistances apparently were only measured to about one per cent. in most cases, and the temperatures are only given to the nearest degree."

2. Another objection occasionally urged is the change of resistance found in platinum filaments when used in incandescent lamps. A little reflection, however, will show that the conditions are in no way similar. Callendar observes on this point ((9), p. 105): "The sudden heating and cooling of the wire when the current is turned on or off, and the intense radiation which keeps the surface at a lower

temperature than the central portions, must be a severe strain on the wire. It is also evident that any crack or flaw in the surface will tend to be intensified by the local development of greater heat, and if the wire is heated to a temperature near its melting point where it begins to be appreciably volatile, this action must inevitably produce serious results. If a wire which has been thus treated be examined under the microscope its surface will generally be found to be cracked and scored in a manner which is in itself amply sufficient to account for the increased resistance and brittleness."

With the exception of objections based on the apparent complexity of the instrument (which I deal with later) the above are the only serious adverse criticisms which it is necessary to notice.

Before adducing the evidence on the other side, it would be as well to briefly explain the terminology which has been found to be convenient. Callendar in 1887 ((3), p. 163) suggested the use of the term "platinum temperature" to denote the reading on a scale so constructed that a rise of 1° of that scale would at any temperature increase the resistance of a platinum wire by $\frac{1}{100}$ of the difference between its resistance at 100° and at 0°C. Hence if R is the resistance of a platinum wire at a temperature t (measured in degrees Centigrade by the air thermometer), and if R_1 and R_0 are the resistances of the same wire at 100°C. and 0°C. respectively, and if pt stand for the platinum temperature, then

$$pt = \frac{R - R_0}{R_1 - R_0} \times 100.$$

In the same paper Callendar communicated to the Royal Society the results of an exhaustive series of comparisons of the resistance of a platinum wire and the temperature as indicated by the air thermometer in whose bulb it was wound. No one can, I think, rise from the study of this important communication without admitting that the experimental evidence produced was sufficient to establish the following conclusions (p. 161, *ibid.*):—

(*a*) The self-consistency of the platinum thermometer

has been abundantly verified by the experiments. If the wire is pure to start with, its resistance is always the same at the same temperature.

(*b*) The relation between the platinum and the air temperature is closely represented by the parabola—

$$t - pt = \delta\left\{\left(\frac{t}{100}\right)^2 - \frac{t}{100}\right\} \quad . \; . \; . \quad (d).$$

The experimental evidence given in support of (*b*) would be sufficient to establish the necessary relation provided we could make certain that all platinum wires used should be of the same degree of purity as that used by Callendar. It appeared probable, however, that a greater or less degree of purity might entirely alter the character of the curve.

In the autumn of 1889 I commenced a series of determinations of certain freezing and boiling points by means of platinum thermometers. At that time I had not had the advantage of reading Callendar's paper, nor was it brought under my notice until my observations were nearly completed; the inquiry was therefore conducted on independent lines. In order to standardise my instruments I had assumed the following "fixed points" in addition to those given by melting ice and steam at 760 m.m. :—

B. P. of	Naphthalene at 760 m.m.	=	218·06	Crafts
,,	Benzophenone ,,	=	306·08	Crafts
,,	Sulphur ,,	=	448·38	Regnault.

I standardised eight thermometers by means of the above five points. These thermometers were of different patterns, their coils were formed of different specimens of platinum wire differently insulated, although the majority of the coils were wound on calcined asbestos and enclosed in hard glass tubes. The conclusions I arrived at may be summarised as follows ((7), p. 64):—

(*a*) The readings of each thermometer are constant when the temperature is the same.

(*b*) Different thermometers whose coils are formed of different specimens of platinum do *not* give the same platinum temperature when at the same actual temperature.

It will thus be seen that my independent investigation agreed as to (*a*) with the conclusion arrived at by Callendar, and was in direct contradiction to the report of the British Association Committee with regard to the Siemens pyrometer in which the wire was insufficiently protected. Conclusion (*b*) appeared discouraging, as it involved a separate and difficult standardisation of each thermometer before use.

On comparing my results with Callendar's it was evident that the upper portions of my $t - pt$ curves departed considerably from the parabolic form, and we, therefore, appeared to differ in our conclusions. The difference was so marked that I consulted with him, and we decided to make a thermometer similar to mine out of a portion of his original coil (which he had fortunately preserved) and expose it to sulphur vapour under the same conditions as those prevailing during my own experiments. The result, assuming formula (*d*) and Callendar's previously found value of δ, gave the boiling point of sulphur as 442·3°C., nearly 6°C. below Regnault's number. It was therefore evident that either Callendar's value of δ was wrong, or that the thermometer in my apparatus did not attain the temperature of sulphur vapour, or else that Regnault's value of the boiling point of sulphur was too high. The matter appeared so important that the summer of 1891 was devoted by us to its investigation, and an account of the work will be found in (8). We believe that we then established the following points : (*a*) that Callendar's value of δ, as obtained during his experiments in 1887, was practically correct ; (*b*) that the bulb of the thermometer in my apparatus did not attain the temperature of the sulphur vapour unless suitably screened and that the error due to this cause might amount to 1·05°C. ; (*c*) that Regnault's value of the boiling point of sulphur was too high, the results by our air thermometer determinations being 444·5°C. as against Regnault's 448·4°C.

The curves obtained by me in 1890 were now re-drawn, substituting *our* value of the boiling point of sulphur for Regnault's. A study of those curves then led to the following important conclusions :—

(*a*) That although the values of δ varied greatly, the curves remained practically parabolas.

(*b*) That the assumption of the parabolic form and of the respective values of δ obtained by observations in sulphur vapour gave values for the boiling points of mercury, benzophenone and naphthalene, which were practically the same whatever the sample of the wire used, provided it was of ordinary commercial purity.

(*c*) That the boiling points of benzophenone and naphthalene, as deduced in the above manner from the numbers obtained from my experiments of 1890, agreed closely with the values given by Crafts ; for example—

	Griffiths	Crafts
Naphthalene . .	217·94°C.	218·06°C.
Benzophenone . .	305·82°C.	306·08°C.

and to appreciate the value of the results it must be remembered that they depend entirely on the validity of the formula (*d*) and on the correctness of the boiling point of sulphur as determined by us in 1891. A reference to the original paper will show that these conclusions were borne out by determinations of the boiling points of methylsalicylate, triphenyl-methane, mercury and the freezing points of Sn, Bi, Cd, Pb and Zn. True, the determinations of the air temperature of these points by previous observers differ very greatly from each other, and none appear to have been determined with the care and accuracy which distinguished the work of Crafts ; but the importance of the comparison lies in the fact that platinum thermometers, whose constants differed greatly, gave almost identical values of these temperatures, which ranged from 184° to 445°C. As it was possible that the departure of the $t - pt$ curve from the parabolic form might become marked at ordinary temperatures, an elaborate comparison was made between a platinum and an air thermometer at every 5° from 0° to 100°C. A table of the results will be found in paper (8) (p. 155). This series of experiments led to the same conclusions as the observations at higher temperatures.

It thus appears that a complete standardisation of a platinum thermometer can be made by observation of its resistance at three temperatures only, and the ones selected by us for the purpose are 0°, 100° and 444·5°C. (sulphur vapour at 760 m.m.). Great care must, however, be taken that the thermometer, when in sulphur vapour, is thoroughly screened as described by us ((8), p. 143). Our experiments with various specimens of sulphur indicate that the sample used need not be of extreme purity, the ordinary impurities not affecting the temperature of the vapour when the boiling has been continued for some time. Thus R_s, R_1 and R_0 being known pt_s can be obtained, and hence the value of δ for the particular sample of wire used can at once be deduced from formula (*d*). The standardisation can now be regarded as complete, and we have an instrument whose fixed points do not change, provided that the wire has been carefully annealed, no matter what temperature it has been previously exposed to, and whose readings are independent of position and external pressure. Its capacity for heat is small, it can be made of almost any size, so as to give the mean temperature of a space, or the temperature "at a point," it can be placed interior of any apparatus and read at any convenient distance. Also, if, as should invariably be the case, the stem is supplied with double electrodes, the readings are uninfluenced by the temperature of the stem or leads. I derived such confidence from the experiments performed with platinum thermometers, whose constants had been determined in the manner I have described, that I standardised the mercury ones used during my determination of the mechanical equivalent of heat by platinum thermometers only. At the close of that investigation, those mercury thermometers were carefully compared with the standard supplied by the International Bureau. The results of the comparison are given in detail ((11), p. 430) and show that in actual elevation a difference of 0·005°C. was found but that the value of temperature ranges was practically identical.

Again, a further comparison was made by Callendar and myself during the summer of 1893, between a new form of air thermometer, whose indications were independent of

changes in the external pressure[1] and the Paris, and some platinum standards. A summary of the results will be found in paper (13), and they but more firmly establish the conclusions previously arrived at.

There are indications that the formula (*d*) gives the relation between the air and platinum scales at very low temperatures. In a communication to the *Phil. Mag.* (10) it was pointed out that we can find at what platinum temperature the resistance of platinum will vanish by placing 0 for R in the formula

$$pt = \frac{(R - R_0)}{R_1 - R_0} = 100$$

and then by using the value of δ peculiar to the wire and assuming the parabolic formula to hold over so large a range, we can deduce the corresponding value of *t*. The method was applied to all those thermometers whose constants had previously been published and the mean result gave $-273\cdot9$°C. The experiments of Professors Dewar and Fleming (12) led them to the conclusion that the electrical resistance of platinum wire would vanish at absolute zero, and thus we have what seems to me very strong evidence of the accuracy of the (*d*) formula at low temperature.[2]

As regards temperatures exceeding 700°C. there is no reason to suppose that the departure of the $t - pt$ curve from the parabola becomes important. It is difficult to carry the comparison with the air thermometer above the temperature already obtained. What we require for high temperatures, however, is not so much the measurement on the air scale, but some practical standard to which they may be referred, and even if the relation between *t* and *pt* at

[1] Callendar, *Roy. Soc. Proc.*, January, 1891.

[2] In cases where a high order of accuracy is not a necessity, a yet simpler mode of graduation is thus suggested. Having found the value of R_1 and R_0 if we assume $R = 0$ when $t = -273\cdot7$°C. we can obtain an approximate value of δ and thus dispense with the observations in sulphur vapour. The results obtainable by this simple method are but approximate. In seven thermometers whose constants were given in paper (10) the greatest error caused by the adoption of this method amounted at 150° to $\cdot44$°C., but if the thermometers are intended for use between 0° and 100°C., the probable error would not exceed $\cdot05$°C.

such temperatures differs considerably from the results obtained by exterpolation the platinum thermometer yet supplies a standard to which all such measurements can be referred. Thus, as far as regards range, the advantages are entirely on the side of the platinum thermometer. It is true that mercury thermometers can, with proper precautions, be used over a very considerable range of temperature, especially those excellent instruments constructed by Niehls of Berlin, which contain gas at a high pressure. Their great drawback, as far as accuracy is concerned, is the difficulty with regard to the stem temperature, a matter of great importance in thermometers of this description. The circumstances under which they are used almost invariably prevent the complete immersion ; thus an estimation of the mean stem temperature is a matter of great difficulty, and however accurately they may have been graduated by the makers, it is hopeless to expect accuracy of a higher order than 1° or 2° C. The same remarks apply to the potassium-sodium alloy thermometers, and the latter I find are in addition very subject to changes in zero after exposure to high temperatures.

The cumulative weight of the evidence I have summarised in this section is, I think, great, and it is discouraging to find that so little use has as yet been made of it in the scientific world. In many cases manufacturers have shown themselves ready to take advantage of so simple a method of estimating high temperatures, but a platinum thermometer is, as yet, rarely to be found in a physical laboratory. True, Messrs. Heycock and Neville have applied the method with complete success during their investigations into the melting points of alloys (5), and Professors Dewar and Fleming have used it to a certain extent in their determinations of low temperatures (12), although the last-named observers evidently distrusted the relation given by the (*d*) formula and have stated their results in the platinum temperature-scale. Such examples of its application, however, are rare, and in no case (except in Callendar's work and my own) do I find any record of its use for the *accurate* determination

of ordinary temperatures, for which purpose I consider it well suited.

With regard to the difficulties of making observations with this instrument, those who urge this point evidently suppose that in order to obtain a temperature by a mercury thermometer it is merely necessary to observe the position of the top of the mercury column, and if they desire no greater accuracy than is obtainable without further exertion I admit that simplicity *is* on the side of the mercury thermometer. Where accuracy is required I think that I have shown in Section I. that the use of the mercury thermometer is by no means simple, and that when we bear in mind the labour involved in the previous processes simplicity is all on the side of the platinum thermometer.

As to the difficulty of constructing the instrument, I am not aware that observers usually manufacture their own mercury thermometers, and since platinum thermometers are now supplied commercially the objection bears but little weight.

In conclusion, I would again quote from Callendar's paper (9): "I quite admit that it requires some special skill and experience to *make* a good thermometer, but the rest of the apparatus required is obtainable in almost any laboratory and it is easy to take the readings quickly and accurately after a little practice. The great superiority of the platinum thermometer in range, accuracy and durability will be found in the end to save as much time and expense as will more than compensate for the small trouble of learning to use it."

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E. H. GRIFFITHS.


BRITISH
NEW GUINEA
Astrolabe B.
Finsch H.
Huon G.
Strickland R.
Palmer R.
Fly R.
Fly R
TUGERI
Moreland R.
Kussa R
DAUDAI
DABU
Boigu
Saibai
Mowatta
Parema
Tagota
KIWAI
Wabuda
Bamu R.
Turama
Tumu
Aird Hills
C. Blackwood
Alele R
Orokolo
Gulf of
Papua
TOARIPI
Motumotu
MAIVA
MEKEO
C. Possesion.
RORO
Hall Sound
DOURA
KABADI
KUPELE
KOIARI
KOITAPU
MOTU
Pt Moresby
Owen Stanley Range
LOYALUPU
AROMA
Cloudy B.
MAIRU
Orangerie B.
DAHUNI
(S. Cape)
MILNE B.
E. Cape
Dawa
Moratu
Collingwood B.
D'ENTRECASTEAUX GROUP
TROBRIANDS
Kiriwina
Murua
Nada
MORESBY GROUP
Wari
Misima
LOUISIADE GROUP
Roua
Tagula
Erub
Mer
Tud
Yam
Mabuiag
Badu
Moa
Nagir
Muralug
C. York
0 15 50 75 100
Nautical Miles

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THE ETHNOGRAPHY OF BRITISH NEW GUINEA.

I. THE ETHNOLOGICAL PROBLEMS.

IN the following pages I have endeavoured to put into a small compass the present state of our knowledge about the largest island in the world. The western half of New Guinea (long. 141°E.) belongs to the Netherlands, of the remainder the Germans have appropriated the northern half as far south as lat. 8°, while the English have annexed the southern half with all the neighbouring islands. The ethnography of this vast island is most interesting; but it is somewhat difficult to master, as there are so many books and papers bearing upon the subject, some of which are erroneous, others are worthless from this point of view, many contain a few interesting facts and much padding, while not a few are of great value.[1]

Many of those who have written on the natives of British New Guinea have not sufficiently distinguished between the numerous tribes in our possession, and they speak in vague terms of the Papuans as if they were all alike. Now this is by no means the case, and before we can gain an adequate comprehension of Papuan ethnography and ethnology we must clearly distinguish between the charac-

[1] The concluding section of this article, which will appear in an early number of "Science Progress," consists of a "Guide to the Literature on the Ethnography of British New Guinea". The references in the present section are to the bibliography in the following section.

teristics of the various tribes, their customs, languages and handicrafts.

There is still much discussion concerning the limitation of the term Papuan as applied to people, and even whether it should not be dropped altogether, as Prof. Sergi suggests. The Italian anthropologist extends the term Melanesian not only to comprise the natives of all the Western Oceanic islands including New Guinea and the adjacent islands but also Australia. At present I adhere to what Mr. Ray and myself have considered to be the most convenient course, and employ the term Papuan for what appear to be the autocthones of New Guinea. By Melanesians we understand the present inhabitants of the great chain of islands off the east of New Guinea and extending down to New Caledonia. These terms are used to designate peoples, not races; neither are pure races, and at present we are unable to gauge the amount of race mixture in either, or even to state precisely what are their components.

From the boundary of Netherlands New Guinea to Cape Possession on the eastern coast of the Papuan Gulf, and inland from these coasts, the natives are dark, frizzly-haired Papuans, typically they are a dolichocephalic people and rather short in stature.

The Papuans also occupy the greater part of the south-east peninsula of New Guinea; but along the southern coast line almost uninterruptedly from Cape Possession to the farthest island of the Louisiades is an immigrant Melanesian population, about whom I shall have more to say presently.

I will now enumerate a few facts which will clearly bring out the essential distinction between these two peoples.

We have not at present a sufficient amount of data on the physical characters of the two peoples by skilled observers to enable us to formulate what differences there may be between them. There is no doubt that the Papuans are more uniformly dark than are the Melanesians (I am now referring solely to the Melanesians in British New Guinea) and their hair is as constantly frizzly. Among the Melan-

esians light-coloured people, even as light as the true Polynesians, are constantly met with, as are also individuals with curly and occasionally straight hair. The features of the former are coarser and they are often much more prognathous; their skulls, too, are more generally dolichocephalic, whereas among the Melanesians a great variety is found in the cranial measurements, as Prof. Sergi has demonstrated. Judging from my experience of the Western Papuans and from numerous photographs I have seen, the Papuan men usually sit with their legs crossed under them like a tailor, whereas the Melanesians squat, like a Malay, usually with their haunches just off the ground. I do not know whether this rule holds good for the Papuans of the south-east peninsula.

The Western Papuans may or may not scarify their skin as in Torres Straits (Haddon, 1890, p. 366), Daudai (Beardmore, pp. 460, 468) and up the Fly River (Macgregor, *C. A.*, 105, 1890, pp. 43-51), but they do not tattoo; the Melanesians tattoo themselves, especially the women. Tattooing has, however, spread to a certain extent among the Papuan hill tribes of the peninsula, the Koitapu women appear to have thoroughly followed the fashion of their Motu neighbours; amongst the Koiari and other hill tribes it occurs only occasionally. The **V**-shaped chest mark, *gado*, occurs among the Motu and Loyalupu, but not east of Keppel Bay. Among the two former the tattooing lacks symmetry, but in Aroma curved lines become more frequent and asymmetrical figures have a bilateral symmetry with regard to the body. This subject was first dealt with by W. Y. Turner, Lawes followed, but the most exhaustive account is by Finsch (1885, *Samoafahrten*, and in Joest, *Die Tätowirung*).

The houses of the Gulf and Western Papuans are often of great size and contain numerous families, and there appears to be more club-life among the men. The houses of the Melanesians are smaller, each family possessing one. Very characteristic of the Papuans are the houses which are confined to the use of the men. The native name is *eramo*, *elamo* or *dubu*; by the white men they are variously termed

club-houses, sacred- or tabu-houses, temples, etc. These houses are the focus of the social life of the men, and as religion among savages is largely social usage, it is in connection also with these structures that most religious observances are held. Dubus apparently occur in the peninsula, but of these we have no precise information, and it is not recorded whether the carved platforms, which have been described at Tupuselei, belong to the social life of the Papuans of the district or their Melanesian conquerors.

The initiation of lads into manhood is accompanied with sacred ceremonies in some of the Papuan tribes, but so far as is known by none of the Melanesians in New Guinea. I have described the initiation ceremonies which took place in Torres Straits (1890, pp. 408, 432; 1893, p. 10), and in my forthcoming memoir (1894) I have quoted the information collected by Chalmers in the Papuan Gulf (1887, p. 85; 1890, p. 312). Masks are usually, perhaps invariably, worn at these ceremonies, and the bull-roarer is swung and shown to the lads; quite recently Chalmers has discovered the bull-roarer about the estuary of the Fly River as well as farther east (1887, p. 85; 1890, p. 313). The bull-roarer was also used in Torres Straits in connection with fishing and probably to raise a wind (Haddon, 1893, p. 20), now in some islands it is a children's toy (Haddon, *Jour. Roy. Inst. Gt. Britain*, 1890). There is no record of a bull-roarer among the Melanesian folk, but it occurs in the German territory (*Zeits. für Ethnol.*, xx., 1888 (Abhandl.), p. 267.

Kabadi and Nara girls about twelve or thirteen years of age are secluded for two or three years, they are sheltered from the sun and never allowed to descend from the house (Chalmers, 1885, p. 159; 1890, p. 319; *cf.* J. G. Frazer, *The Golden Bough*, ii., p. 228).

After initiation the lads rank as men and are allowed to marry if they can afford the bride price. Chalmers says the Gulf men have to pass several stages before the carved wooden belts can be worn; he also informs us that only old men have seen the sacred image of *Semese*, "and various are the initiatory steps before it can be seen" (1887, p. 86).

Masks are employed by many peoples during certain

ceremonies; their distribution in New Guinea is interesting, as it will be found that in the British Possession they characterise the Papuan as opposed to the Melanesian elements. They were common in Torres Straits, have been obtained in Daudai and are very abundant in the Papuan Gulf from Maclatchie Point to Cape Possession, they are probably in use along the remaining portion of the Gulf; the most southern limits at present known are the Maiva and Mekeo districts (St. Joseph River). Wilfred Powell, however, bought at Rogea (Heath Island) a turtle-shell dancing mask, which he was informed came from Mount Thompson on the mainland (long. 149° 50′E.); he describes and figures it on p. 16 of his *Wanderings in a Wild Country* (1883). This is the sole recorded exception to the statement just made, and must remain so pending further investigation. One or two specimens have been obtained from the neighbourhood of the Kaiserin Augusta River. Zimmermann says he saw them from Geelvink Bay to Humboldt Bay (*Internat. Arch. f. Ethnogr.*, ii., 1889, p. 53; *cf.* also Clercq and Schmeltz, p. 240). The Torres Straits and Daudai masks are made of wood or turtle-shell, those of the Papuan Gulf of a natural cloth which is painted and the designs are limned in cloissons; some of those of the German territory are somewhat similar to the latter, but the cloissons are, I believe, absent. What is known as to the occasions on which these masks are used will be found in Haddon, 1893 and 1894; in the latter Chalmers and others are quoted in full. In all cases masks are ceremonial and can only be worn during the ceremony. Photographic illustrations of eight Torres Straits masks, with careful descriptions, will be found in Meyer's *Masken*, etc. (1889), as well as of thirty-four small wooden face-masks from Taraway (Bertrand and Guilbert Islands), Kaiser Wilhelm's-Land (*cf.* also Partington's *Album;* Andree, R., "Die Masken in der Völkerkunde," *Arch. f. Anth.*, xvi., 1886, p. 477; *New Guinea*, p. 497). The Taraway masks are probably connected with ancestor-cult, like the *Karwars* of Netherlands New Guinea (*cf.* also *J. A. I.*, xix., pp. 319, 320).

Dancing may be a secular amusement or a ceremonial

exercise; in both aspects it is largely practised by the Papuans proper; the Melanesians do not appear to dance much, and then only for amusement. Religious dancing is not practised.

A classification and description of various kinds of dances in Torres Straits is given by Haddon (1893). Romilly (1889, p. 88) says: "In the south-eastern archipelago dances are very rare, nor have I ever seen one. Such dances as there are are only performed after a feast, no superstitious importance is attached to them; . . . but to the westward, in the Gulf of Papua, symbolic dances are constantly performed. . . . Each man is dressed to represent some bird or fish, and the dresses they appear in are marvels of ingenuity and construction. The shark is a very favourite symbol, as are also wild ducks and geese." A Motu dance is described on p. 54, and by Stone (p. 193), who also describes one at Hula (p. 195) and says: "Among the Motu the unmarried of both sexes join in the dance, but among the Kerapuno only the men"; but women dance close by at Kalo on some occasions (*Arch. Rev.*, 1890, p. 149), and Bevan (1890, p. 128) on an islet close by Dufaure Island saw men and women dancing surrounded by imitative children. Romilly notes (p. 60) a war-dance at Cloudy Bay. Chalmers (1887, p. 181) compares Motu and Motumotu dancing customs.

Social organisation is very simple in British New Guinea, there is no priestcraft or statecraft. In Torres Straits I discovered totemistic clans, which, however, appeared to be of diminishing importance, nothing of the sort has been described from elsewhere. The people are grouped into villages, and in some cases the inhabitants of various villages recognise one another as belonging to the same tribe. In the Port Moresby district members of the Koitapu and Motu tribes may live in apposition but not in fusion in the same village. Throughout the whole of the Protectorate chieftainship in the true sense of the term is absent; a man may acquire personal power through bravery, wealth or some other cause, but it is not hereditary, and even so these so-called "chiefs" have very little influence even

in their own village. The policy of the "Government" is to alter this in order to make some one more responsible for order in his district. The only exception to this state of affairs is in the Trobriand Group, where Macgregor found chiefs who exercise real authority.

Women take a good position in British New Guinea and exercise considerable influence over the men, less so, however, in the Trobriands.

In the foregoing social characteristics, in their lack of hospitality and in most of their mental traits the Papuans and Melanesians resemble one another, but the Papuans appear to be more noisy and excitable.

Of their weapons the stone-club is alone common to all the tribes. The use of the bow and arrow is confined to the Papuans and is universally employed to the west and in the Papuan Gulf, about Cape Blackwood the bamboo bow of the west is replaced by the palm-wood bow. Macgillivray (i., p. 29) alludes to the bow and arrow of Redscar Bay, Gill (1876, p. 249) says: "Their bows are made of wood, not of bamboo, as on the south-west coast, and are very inferior articles," and Moresby writes (p. 157): "As far as Redscar Bay (coming from the west) we saw toy bows amongst the children, but beyond that point the bow ceases altogether, till it reappears on the northern shores, west of Astrolabe Gulf". Turner (1878, p. 488) alludes to the bow and arrow of the Motu; but this is a mistake on his part.

Heavy, sword-like, wooden clubs are common among the Melanesians, and the sling is employed in the D'Entrecasteaux Islands. The throwing stick and javelin are used only by the western tribe of Torres Straits, who have adopted the practice from the Cape York natives.

Only the Melanesians make pottery, it is made at Yule Island (Navarre, p. 304) and at various places along the coast as far as Aroma (Gill, 1876, p. 249; Turner, 1878, p. 489; Chalmers, 1887, p. 23). In the islands off the south-east peninsula the pots are made in a different manner, the clay being laid down in a spiral and no stone and beater are

used.[1] Mr. Ray draws my attention to the fact that pots are called by the same name in Melanesia and British New Guinea (Fiji, *kuro;* Espiritu Santo, *kuro;* Motu and Maiva, *uro;* Kerepunu, *gulo*, etc.).

The Papuans earlier adopted tobacco and grew their own tobacco before the white man came, but they do not chew the betel to any great extent, quite the reverse is the case with the Melanesians. Strangely enough kava is only known along the Fly River and by the Masingara, a Daudai bush tribe.

In a memoir which is now in the press I discuss with considerable detail the decorative art of British New Guinea. I have been able to distinguish the following artistic provinces: i., Torres Straits and Daudai; ii., Fly River; iii., the Papuan Gulf; iv., Central District (*i.e.*, south-east peninsula from Cape Possession to Cloudy Bay); v., the Massims, including the district around Milne Bay and all the neighbouring island groups. The first three districts are purely Papuan, as is the bulk of the decorative art of the fourth, the Melanesians of this district appear to be an inartistic people; quite the reverse obtains for the island Melanesians, who are characterised by great skill in wood-carving and by the employment of varied and beautiful scroll designs. As this subject cannot be discussed in the absence of illustrations I must refer the reader to the original memoir.

I have now enumerated a sufficient body of evidence to demonstrate that two groups of people inhabit British New Guinea. We have now to see whether a further analysis is possible.

Our knowledge of the Western Papuans is too imperfect for any definite generalisations to be made at present, but I venture to present the following tentative suggestions:—

The most typical Papuans in the British Protectorate are probably the bush tribes from the Dutch boundary to the back of the Gulf of Papua. They are gradually being pushed

[1] *Cf.* Finsch, "Töpferei in Neu Guinea," *Zeits. f. Ethnol*, xiv., 1882, p. 574; *Samoafahrten*, p. 281; *Atlas*, pl. iv., figs. 6-10; also H. O. Forbes in Haddon, 1894.

inwards by the coast people. Macfarlane contrasts the high and broad skull of the latter with the "long, narrow skull, with its low forehead and prominent zygomatic bones," of the former, whom he also states are "greatly inferior, both mentally and physically". The observations of D'Albertis[1] of a racial mixture in this region are supported by Quatrefages and Hamy (p. 256). The Torres Straits islanders are also a mixed people. I do not think we have sufficient evidence before us to decide what are the component races of these Western Papuans. I suspect that the Fly River is to a slight extent what may be termed a "culture route" and that the natives of the higher reaches have indirect communication with those of the north coast of New Guinea; for example, the rattan armour collected by D'Albertis high up the river is similar to that obtained by Finsch from Angriffs Havn, near Humboldt Bay, and recalls the coir armour of Micronesia; it is probable that this was the route by which tobacco found its way to Torres Straits and the Gulf district, and thence to the south-east.

The Papuans also extend down the south-east peninsula and into the adjacent island groups. On the mainland they have been conquered in certain places by Melanesian immigrants, and a mixture of these two peoples has taken place to a variable extent. In the islands the amalgamation has been more complete.

The immigrant people are by the great majority of writers spoken of as Polynesians; this identification is apparently based solely on the lighter colour of some of the former than that of the Papuans proper, and numerous words common to them and the Polynesians.

The light colour of the skin and the occasional presence of curly or even straight hair among some of the people of British New Guinea certainly proves a racial mixture, although Comrie and Finsch do not lay much stress on these points. The latter (1888, p. 234) writes: "The natives of Bentley Bay, as at East Cape, are of a tolerably

[1] ii., pp. 377, 381; *Jour. Anth. Inst.* vi., 1876, p. 216.

light skin colour and belong to what the ignorant would explain as a Malay mixture. But wrongly, for they are true Papuans, amongst whom the individual occurrence of curly, even of smooth hair is of no consequence." The craniology of the natives of the south-eastern peninsula and neighbouring islands has been studied by Comrie, Flower, Miklouchо, Maclay (*Proc. Linn. Soc., N.S.W.*, vi., 1882, p. 171), Quatrefages, Hamy and Sergi, most of whom admit with Flower "a considerable mixture of races among the inhabitants of this region of the world". As at present anthropography cannot speak with precision concerning the racial elements in this immigrant people we must turn to other branches of anthropology, and we will see what light ethnography and linguistics can throw on this ethnological problem.

A comparison of Papuan and Melanesian customs and handicrafts will prove that there is little of real importance in common, say, between the Motu or the South Cape natives and the Samoans. I need only allude to the almost total absence of a system of cosmogony or of a pantheon with a definite mythology; associated with this lack of a theology is the absence of an organised priestcraft. The democratic Papuans and Melanesians have no hereditary chieftainship, and the power of tabu is much more limited than in Polynesia. Strangely enough these so-called "Polynesians" in south-east New Guinea make pottery and do not drink kava.

For the linguistic evidence I have consulted my friend and colleague Mr. S. H. Ray, who is our great authority on the languages of Western Oceania, and the following account embodies certain conclusions, which he has kindly allowed me to quote from his unpublished MSS.: "It must be accepted as an axiom in philology and ethnology that the direction from which an observer approaches unknown languages or peoples will materially influence his description of them. The languages and customs of a newly discovered people will naturally be compared with one better known, and if there be any considerable amount of apparent agreement the conclusion will probably be hastily arrived

at that there is a connection between the newly found and the well known, and there will be a decided tendency to adopt the old as a standard and to refer the new to it. Those who first became acquainted with any New Guinea languages came from Samoa and Niue, where is found the fullest and most complete form of Polynesian speech." The numerous words which are common to the Motu (which is the only language of this district of New Guinea which has been thoroughly studied) and Polynesians "are also common in the Melanesian region. This leads to a further inquiry: Did the Motu and the Melanesians receive these words from the Eastern Polynesians? Before answering this it is necessary to observe that the Motu contains numerous words which are in the Melanesian of the Melanesian islands and do not appear in Samoan. A comparison of vocabularies shows that the Motu and the Melanesians stand in the same relation to the Samoan, whatever that relation may be." Mr. Ray shows that where Polynesian words diverge from Melanesian it is due to "worn down or abbreviated pronunciations of the fuller Melanesian, and the Motu has preserved a fuller form of certain words than the Samoan and hence presumably an older form. But there is a more important feature in the Motu language of New Guinea which distinguishes it from the Polynesian. Words are not used in the same way. The grammars of the languages are not essentially different, but in the Polynesian there is a tendency towards simplification which is not found in the Motu. It seems that we must regard the Samoan forms as in a later stage than that of the Melanesian. The Motu follows the Melanesian rule and is thus presumably in an older stage than the Samoan. Much more could be written to show that it is with the Melanesian tongues that the Motu of New Guinea should be included and not with the Polynesian. The same method applied to the Kerepunu, the Aroma, Suau, and other dialects akin to the Motu, points to the same relationship. The Motu grammar is entirely Melanesian and non-Polynesian. Such words as are common to it and the Eastern Poly-

nesian are equally common to the whole of Melanesia. Melanesian words which are non-Polynesian are also found in Motu and the allied languages of New Guinea."

I had long been puzzled by certain differences between the Motu and allied tribes on the coast of British New Guinea and the natives round Milne Bay and of the neighbouring groups of islands, all of whom I speak of collectively as the Massim.

There is a difference in their physiognomy. The Motu and allied tribes are remarkably destitute of a religion and are (or were) at the mercy of the sorcerers of the indigenous hill tribes, and, what is more remarkable, there is no trace of the cult of the sacred frigate bird or of that of any other animal. They make their pottery by beating a lump of clay into a pot, whereas, according to the only descriptions we have, the Massim women build up their pots with bands of clay laid on spirals. A study of my memoir on the decorative art of British New Guinea will clearly bring out the enormous difference between the Motu and the Massim in artistic feeling and execution.

My knowledge of Melanesia was too slight to enable me to proceed further with this problem, but in a recently published paper Mr. Ray says (1894, p. 32): "With regard to the place of origin of the Melanesian population of New Guinea it does not seem possible to ascertain the exact quarter from which it has come. There is at first sight much dissimilarity between the languages west and east, between the Motu and Kerepunu on the one side and the Suau of South Cape on the other. Though this dissimilarity disappears on closer examination, it may be stated that the language of Suau appears very similar to those of San Cristoval in the Solomon Islands, which lies almost due east of South Cape. The Motu and Kerepunu agree more with the languages of the Efate district in the Central New Hebrides."

Further evidence must be collected before Mr. Ray's suggestion can be definitely accepted. The decorative employment of the frigate bird in the Massims and Solomon Islands supports his first proposition; but, on the

other hand, inlaying with shell and nacre is very characteristic of the Solomon Islands and this is absent from the Massims, there are besides many other points of difference. So far as I am acquainted with photographs of natives from the New Hebrides I do not see any resemblance between them and the Motu, but it must be borne in mind that there can be culture-drift without appreciable actual mixture, though amongst savage peoples the latter must to a certain extent be concurrent.

When one thinks of the considerable amount of literature there already is on New Guinea one is surprised at the vagueness and imperfection of a good deal of our information. Now that the problems to be solved have been clearly stated it is to be hoped that further and more precise data will be forthcoming.

ALFRED C. HADDON.

WORK ON THE PALÆOZOIC ROCKS PUBLISHED IN 1894.

I. THE BRITISH ISLES.

IN reviewing the progress made in work amongst the palæozoic rocks, one is led to commence with a description of that which has been carried out amongst the rocks of our own country, though at first sight it might appear less necessary to allude to it, than to discuss the less accessible papers of foreign writers; still much of our own contribution lies partially buried in the transactions of local societies, and we have sufficient material to bring together in the form of a connected record, concerning one question of importance and interest to all, namely, the distribution of our coal deposits.

In this article it is proposed to notice such work as was published on the older rocks of our island during the first part of the current year, but the appearance of one paper on "Coal" in the August number of the *Journal of the Geological Society* requires notice out of its turn, as it will be convenient to consider it along with several other papers on this subject published during the earlier part of the year.

Before discussing the work of the first half of 1894, I feel that it is necessary to allude to my last article published in "SCIENCE PROGRESS," where I speak of the zonal method of working, as though it were a *new* method. Such is of course not the case. The subdivision of our strata into zones is merely a more detailed application of Smith's principle of strata identified by their included organisms, and no hard and fast line can be drawn between the early work of the pioneers of stratigraphical geology and the detailed work of the latest observers; indeed one can trace an insensible gradation from the one work to the other, and the later work could never have been done until the earlier was accomplished. Amongst the older rocks, the work of

Sedgwick and Murchison was followed by the more detailed work of Salter, Harkness and others, and especially of Hicks, and it was the fuller observations of these geologists which rendered possible the still more minute work of Lapworth and others, which brought about the results chronicled in my last article. The reference to the zonal method, as though it were a *method* instituted within the last fifteen years, was, therefore, hardly just to previous workers, and I gladly take this opportunity of correcting a statement which may have produced a false impression concerning the value of the researches of those workers, an impression which, I need hardly say, it was not my intention to produce.

Though much has been written upon the palæozoic rocks of Britain and their included fossils during the six months under notice, there is a relative dearth of papers which are strictly stratigraphical; but as many references are made to the characters of the rocks or fossils of the strata, which it is within my province to consider, brief allusion will be made to the more important of them, leaving their fuller consideration for the petrographer or the palæontologist. Papers which bear more or less directly on the nature of the Precambrian rocks are naturally numerous, and will be noticed, even when the Precambrian age of the rocks under consideration is a matter of uncertainty, for though the rocks may not be Precambrian, they may nevertheless throw some light upon the vexed questions connected with Precambrian rocks. Of the palæozoic rocks, we find that the lower division has not been the subject of many memoirs, whilst those treating of the upper division are, as might be expected, chiefly concerned with the carboniferous strata. It will be convenient to consider recent additions to our knowledge under three heads: to treat, in the first place, of those papers bearing upon the Precambrian rocks; next, of those which deal with the lower palæozoic rocks, and finally, of the more numerous ones which are concerned with the upper palæozoic strata.

1. *Precambrian Rocks.*

In dealing with these rocks, it is really the province of the stratigraphical geologist to treat of such as are known to be "stratified" and to leave the still mysterious "schists" alone, but in these days, when the latter rocks have so fascinated the minds of geologists, it is a hard task to pass them by. Perhaps, in the present state of our knowledge, it will be the best plan to point out what work has been done in each British area where rocks have been claimed as Precambrian, alluding only briefly to those regions where the age of the rocks has not been pretty clearly established. In doing so, we must pass along the length of the island from Cornwall to Ross, and may commence in the south, and travel northward, starting with the Lizard district. Awaiting with confidence for palæontological evidence concerning the age of these and other doubtful Cornish rocks, we pause for a moment only to refer to a paper which is rather petrographical than stratigraphical (1), in which the writer notices the existence of ultrabasic, basic, intermediate and acid intrusive igneous rocks, and argues in favour of their differentiation from an igneous magma. Travelling north from thence, we reach a spot where there is more satisfactory evidence of the Precambrian age of the rocks, for the Cambrian rocks are found in their immediate vicinity. This is the Malvern ridge. Here the publications of the six months under consideration deal with petrographical matter also, and a reference to the *Geological Magazine* (2) will enable the student of Archæan rocks to see two sides of a question as to the origin of certain biotites in the crystalline rocks of this ridge. Concerning the Scotch rocks Mr. Barrow has a paper (3), which to a large extent summarises work and views advanced in detail in a former paper which he read before the Geological Society, and a very acceptable contribution to our knowledge of the Precambrian rocks comes in the shape of the geological survey map of the district around Gairloch (4), so long celebrated on account of the relationship between the Torridon sandstone and the gneissose rocks in that region. Of the

older rocks, we find here foliated and massive hornblendic, pyroxenic and micaceous rocks, quartz rocks and quartz schists, limestones and graphite schists included in the legend under the name of "Lewisian Gneiss". These are interbanded with long strips of epidiorite running in a general north-west and south-east direction, and the whole succeeded unconformably by the "Torridonian" group, which the surveyors divide into a division of grey grits, dark shales, and breccias, and another of coarse red and chocolate grits. Lastly, Professor Bonney contributes a paper (5) on general questions concerning the formation of gneiss, in which are remarks bearing upon the formation of, or the occurrence of changes in, the rocks of Twthill (Caernarvon), Llanfaelog (Anglesea), the granite of St. Davids, the "Hebridean" rocks and Torridon sandstone of Scotland, and of rocks referred by some to the Precambrian, those of the Lizard district and of Guernsey.

2. *Lower Palæozoic Rocks.*

Contributions to the history of these rocks have been received from Cornwall and the northern part of England. In the former area Mr. F. Stephens maintains (6) that the rocks of the Marazion and Perranuthnoe districts, like those of St. Just, "must be considerably older than the strata of Camborne and Falmouth, possibly very early Cambrian," though no evidence appears in the paper in support of this view. A more important paper referring to the old rocks of this country is by Mr. Teall (7), who describes the occurrence of spheroidal greenstones with radiolarian cherts (these cherts occur both on Mullion Island, where they have been previously recorded in the publications of the Geological Society of London, and on the mainland in the Gorran and Veryan districts, where they have been subsequently detected by Mr. Howard Fox). On the mainland these cherts have been ascertained apparently to underlie the Caradoc quartzites. Mr. Teall mentions the record by Rothpletz of similar radiolarian cherts in Saxony associated with spheroidal diabases, and a similar association of ig-

neous rock and radiolarian chert has recently been detected in the Ordovician rocks of the southern uplands of Scotland. Jasper is associated with so-called spheroidal basalt in California, and Mr. Teall thinks that this jasper may possibly be a radiolarian deposit. He remarks that it seems "almost impossible to avoid the conclusion that the peculiar structures in question are in some way connected with the physical conditions under which the cherts accumulated," and asks if they may not be characteristic of submarine and possibly deep-sea lavas. Anything tending to throw light on the remarkably interesting palæozoic radiolarian deposits, which have been so largely detected of late years, is of interest, but it may be doubted whether it is not yet too early to generalise on the connection between radiolarian cherts and spheroidal basic rocks. The writer of this article finds in his note-books observations of spheroidal diabases in Bohemia in rocks of Precambrian, Ordovician, Silurian (Llandovery, Wenlock and Ludlow) and Devonian ages. In most cases they have broken along shales, and the association of radio larian cherts with soft graptolitic shales may account in some cases for the existence of the igneous rocks also in that position, for some at any rate of these Bohemian diabases are undoubtedly intrusive.

In a stratigraphical paper (8) on the Skiddaw slates of the North of England, the writer takes advantage of the observations of Harkness, Nicholson, Ward, Goodchild, Postlethwaite and others to summarise our present knowledge of these rocks; he considers that the Skiddaw slate group may consist of a thin series of Arenig rocks interfolded with older ones. He gives a list of graptolites compiled from previous lists with additions, and maintains that the presence of *Bryograptus* proves the existence of Tremadoc beds in the Skiddaw district. The paper also contains an attempt to subdivide the Arenig rocks of the Lake district and adjoining regions. A paper by Mr. Reed (9) contains a careful description of a new species of *Phacops* of the sub-genus *Chasmops* (*P. Marri*), from the Applethwaite division of the Coniston limestone of Applethwaite Common near Windermere. It is compared with other

forms, including the Russian species described by Fr. Schmidt, but the author gives reasons for separating it from these. Mr. Reynolds (10) adds to our knowledge of the fauna of the Lower Palæozoic rocks of Western Yorkshire, by describing a new species of trilobite rare in Britain under the name of *Dindymene Hughesiæ*. It is found in Bala rocks, which have also furnished a cystidean of the family *Anomalocystidæ*, on which the author has notes. He further records several trilobites from the Yorkshire Llandovery rocks which also occur in the Stockdale shales of the Lake district, and two species of *Cyphaspis* resembling continental forms and apparently new to Britain.

3. *Upper Palæozoic Rocks.*

We have few additions to our knowledge of the Devonian rocks, and these are hardly of such a nature as to require notice in an article written for stratigraphical geologists. Consequently in this portion of the article the carboniferous rocks alone will be dealt with, as the Permian rocks also have furnished no new material. Commencing with papers which are mainly concerned with lithological or palæontological details, it will be sufficient to allude, under the former head, to a paper which gives additional information as to the composition of fire clays (11), and under the latter, to a number of writings adding to our knowledge of the botany and zoology of the carboniferous period (12). The oil shales of Scotland receive notice in an American publication (13). They are noted as occurring in an area which is roughly twenty miles in diameter in the neighbourhood of Edinburgh, in the calciferous sandstone group, and have a thickness of 3000 feet. The shales are described, and their changes in the neighbourhood of intrusive igneous rocks, *e.g.*, the production of ozocerite in the Binney sandstone. It is stated that there is little doubt that this owes its origin to the distillation of bituminous matter by igneous intrusions in the vicinity of the overlying oil shale. The shale appears to have been formed in tranquil lagoons, into which vegetable matter was brought down in a very fine state of division, but that the hydrocarbon is

sometimes of animal origin is clear from the vast quantity of cyprids which make up some of the shales. Large plants are rare in these shales, but ferns are abundant.

In conclusion we have to deal with a very important group of papers treating of the distribution of coal under newer rocks, a subject that has received an unusual amount of attention of recent years. The first of these papers on the Forest of Wyre coal-field (14) is of somewhat local interest. It deals with the question of the distribution of the lower coal measures beneath the upper measures of that field. The author insists upon the value of the *Spirorbis* limestone, occurring in the upper coal measures, as a datum line. These upper coal measures, with "stinking" or "sulphur" coal, owing to the existence of the Symon Fault in this basin, rest unconformably upon the lower measures, with "sweet" coal resembling those of Coalbrookdale. He believes that the sweet coals only occur in the twelve thousand acres around Kinlet Knowle, in the Forest of Wyre coal-field, and are absent elsewhere, owing to the "Symon Fault". This "fault" he supposes to run in the direction of Shatterford, and maintains that on that account the "sweet" coals are absent there. The paper is illustrated by a map and sections. The next paper (15) treats of the important subject of the extension of the Yorkshire coal-field beneath the newer rocks to the east (15). It is illustrated with a map showing the outcrops of the Ganister, Silkstone and Barnsley coals in the Yorkshire, Derbyshire and Notts coal-field, and with sections. The author discusses the shape of the whole basin. "It would seem that the centre of the trough or coal-basin runs in a line from north-west to south-east, starting at the north-western outcrop of the coal measures, say at Denholme, about half-way between Halifax and Keighley, and proceeding south-west through Cleckheaton, Batley Carr, Sandal Castle (about two miles south of Wakefield), South Kirby and Doncaster. If this line were continued it would pass through Gainsborough to a point about seven miles north of Lincoln, and farther east to a point three or four miles south of Horncastle." He suggests the possibility of the easterly dip on the north-east

side of this axis being smaller than the western, in which case there would be a considerable extension of the eastern part of the basin below the newer rocks. It may be observed, however, that if the Pennine Chain, as is probable, is one of a series of monoclinal folds of which another is concealed to the east of this coal-field, the chances are that this concealed one has a steep westerly dip, which would considerably curtail the area of the basin in that direction. The author comments on the thickness of the newer rocks above the concealed coal-field, which would depend on the slope of the old post-carboniferous land surface, and the presence or absence of different members of the newer rocks, concerning which we require more information than the limited amount supplied by the Scarle boring. He refers to two other borings, details of which are not yet published: one at Carlton near Snaithe on the Aire, and the other between this and the Scarle boring on the Trent. In the discussion on this paper Mr. C. E. Rhodes, referring apparently to this Trent boring, says that a seam, assumed to be the Barnsley, was proved at a depth of 3300 feet, midway between Doncaster and Gainsborough.

The next group of papers refers to a question of still greater interest, namely, the possible existence of concealed coal-fields in the south-eastern counties of England. Mr. Harrison (16) has issued a pamphlet in three parts, referring to the possible existence of coal beneath the newer strata of Essex. The first part was written in 1887 and the second in 1891, whilst the third part is new. He suggests that the Ardennes-Mendip axis turns northward between Calais and Dover, and then in a westerly direction under London and the Thames Valley to the Mendip Hills, and that the Calais coal may lie to the north and the Dover coal to the south of this axis. Quoting the magnetic observations of Professors Thorpe and Rücker, he refers to the possibility of two ridges of older rock running northward from this axis, from near the town of Reading, the easterly one towards Cambridge, and advocates the desirability of searching for coal to the east of the latter ridge, in North-west Essex, in the neighbourhood of Quendon.

The Rev. A. Irving (17) doubts the existence of coal beneath that neighbourhood: he considers that the Ardennes-Mendip axis runs south of Dover, and that the old rocks beneath London form part of a ridge with a southerly slope, so that the centre of this ridge lay, not beneath London, but some distance to the northward, the older rocks of Ware, etc., forming part of it, and not of a north and south ridge running at right angles to it, and therefore coal is not likely to be found in Essex. He alludes to the carboniferous rocks occurring at Harwich, but considers that they may dip towards the south-east, so that the coal-basin may be under the sea, and not anywhere under the eastern counties north of London.

Messrs. Whitaker and Jukes-Browne (18) think there is nothing distinctive about the Palæozoic rocks reached in the Culford boring near Bury St. Edmunds, but remark that all are agreed that they are older than the coal measures. They believe that the Wenlock beds of the Ware boring are quite as likely to belong to Wenlock limestone as to the Wenlock shale series, and prefer to speak of them, therefore, merely as the Wenlock beds. In a section of the paper containing their general conclusions, they mention two points giving "slight support to the view that the old rock at Culford may be pre-carboniferous, and perhaps pre-Silurian in age. . . . On the other hand, however, the fact that at Harwich, which is nearer to Culford than any other of these deep borings, it is carboniferous slate that has been found, and that this is, in some respects, not unlike the harder parts of the Culford rock, naturally leads the sanguine to hope, if not to expect, that the latter too may be of carboniferous age. Should this view be right, the likelihood of still higher carboniferous and coal-bearing rocks occurring underground somewhere in the eastern counties is, of course, greatly increased." The authors also state that we can hardly expect that the northern rise of the older rocks continues underground beyond the neighbourhood of Culford. In the discussion upon this paper Prof. Boyd Dawkins called attention to the fact that the non-discovery of coal-bearing rocks in the area under consideration did not

prove the non-existence of coal-fields in the London basin, but merely of a pre-carboniferous region analogous to that separating the South Welsh and Forest of Dean coal-fields, or those of Gloucestershire and North Somerset. The Culford shale seemed to him probably of Silurian age. Professor Judd was not sure that the beds occurring at the bottom of the Richmond boring might not be abnormal carboniferous rocks, like those of the Northampton borings. The late president of the Geological Society, in the annual address, alluded to in my last article (19), gives a summary of our knowledge of the carboniferous rocks of the Dover boring, and remarks that "although the prospects of finding coal in the East Anglian Palæozoic area are not very bright, it is just possible that the adventurers may strike the coal measures in one or other of the narrow synclinal troughs running east and west in Essex, Suffolk and Norfolk". He himself is "disposed to agree with Mr. Brady that, in further explorations for coal beneath the secondary rocks, the southern alternative of Prestwich is the one which holds out the greatest hopes. It will be tolerably safe to assume that future operations should follow a nearly direct westerly course from Dover towards Bristol."

A few observations on the general question of the discovery of coal-fields in South-eastern England may not be out of place here.

The symmetry of the North of England coal-fields is very marked, though modified by a slight circumstance which produces a considerable deviation from true symmetry. The intersection of the Pennine anticline and Rossendale anticline nearly at right angles to one another produces a cruciform arrangement of coal-basins. On the east of the Pennines we have the Northumberland and Durham and the Yorks, Derby and Notts coal-fields, and on the west the Whitehaven and the South Lancashire and North Staffordshire. The dome of old rocks of the Lake district destroys the symmetry by an extensive modification of the Whitehaven coal-field, though the modification is perhaps not so marked as it appears, for these coal measures probably occupy a considerable area below the newer rocks of the Eden Valley.

Again the coal-fields of Southern England probably have a fairly symmetrical distribution on either side of the Mendip axis. But in Central England the case is different. It cannot be assumed that the Pennine Chain and the corresponding supposed anticline to the east of the Northumbrian and Yorkshire coal-fields run south to join the easterly continuation of the Mendip ridge, for their course would probably be profoundly modified by the existence of the old pre-carboniferous ridge running in a general east and west direction through Central England, which caused the absence of the lower carboniferous beds in that area. Such a barrier would certainly affect the subsequent folding, and as a consequence of it we actually see a marked want of symmetry in the arrangement of the exposed coal-fields of the midland counties. This want of symmetry might well, and probably would, extend to concealed coal-fields. Now the central ridge, according to Professor Green and others, should extend towards Bury St. Edmunds, and the old rocks of the Culford boring may be a portion of it. If this be the case, it would be very hard to predict the probable position of coal-fields in East Anglia, and indeed it seems to the writer that nothing short of an extensive series of borings can settle the question, and that advice as to the stations where borings should be tried is valueless, except in so far as it concerns the possible approximate thickness of rocks overlying the Palæozoic floor. The influence of this central ridge would not affect the old rocks south of the Ardennes-Mendip axis, and this seems an additional reason for advising trials to the south rather than to the north of that ridge. In the meantime the thanks of geologists are due to those who are patiently collecting the records of all borings and gradually aiding us in our restoration of the old buried palæozoic floor of South-eastern England.

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THE RESERVE MATERIALS OF PLANTS.

AT first sight the story of the metabolism of the vegetable organism seems to be that of a series of much simpler operations than those of the animal one. The food on which it subsists is presented to it in a very simple form, and the elaboration of the plant's substance from such compounds as are offered to it by the atmosphere and by the soil involves such a vast amount of constructive change that we may almost be pardoned for overlooking anything of a different nature. Yet it is equally true that vegetable protoplasm, like animal, is quite incapable of using these simple materials for its own nutrition and increase, in the forms in which we see the plant as a whole taking them in. The constructive changes, which we can trace, lead to the formation of vast numbers of bodies of various degrees of complexity and different organic constitution. But this construction must precede the actual nutrition of the living substance, and the food-stuffs on which the latter really depends are at least as complex as the food on which the animal lives. The structure of the vegetable organism favours the absorption of the simplest substances, and that chiefly because it is impossible for the elaborate food-stuffs of animal life to enter it. But these unelaborated materials do not nourish the plant without much expenditure of energy upon them. The essential similarity of the ultimate processes appears evident when we consider such organisms as the group of the Myxomycetes, where an approximation to the conditions of animal life is attended by a similar mode of absorption of food material.

The metabolism of the plant consists then of two essentially different processes. The simpler bodies originally absorbed are worked up at the expense of a great out-put of energy into bodies such as those on which an animal lives, and it is only after such formation that the actual nutrition of the organism can be said to commence.

The needs of the organism are again very similar. The

repair of the actual framework is not so great as that of the animal, but the continual formation of fresh material is comparatively greater. Growth, though slow in plants, taken as a whole, is nevertheless much more prolonged, and year by year sees also the formation of new substance to replace that cast off by reason of the changes of the seasons. The conditions of life are so different and the intermittence in the absorption of material so much more irregular that we must expect the whole story of metabolism to run upon different lines, though the ultimate nutritive processes may show a very close correspondence. The tendency of plants is to accumulate stores of material on which their protoplasm may subsist and from which they may construct their new substance. Long periods often occur in their lives during which their constructive powers are in abeyance, and as then no absorption of their food-stuffs can take place they must subsist on the stores they have been able to accumulate in more favourable times. The materials which thus are found deposited in their tissues have been called generally their *reserve materials*, and a great part of their metabolism is directed towards an accumulation of these.

We are apt, perhaps, in using this term *reserve materials* to think only, or chiefly at least, of those stores which we find laid up in seeds, tubers, or other parts to carry them over a period of absolute quiescence. This thought, however, falls short of the actual needs which they show. When we consider the processes of growth and repair we recognise that these are largely local and that the localities are many and widely distributed in the organism. The activity varies very much in these various regions from time to time, and with this variation arises the necessity for a continual circulation of the nutritive material about the plant. Besides the stores of food-stuffs deposited in the various reservoirs we can discover, we must recognise a circulating store, flowing sometimes in one direction and sometimes in another, consisting to a certain extent of the same materials as the quiescent reserves, but generally in a more plastic form, fitted, that is, for the immediate use of the living substance. Indeed the utilisation of the deeper reserves depends upon

their transformation into the forms found in the circulating fluid, under the influence of various ferments and other agents and under the control of the protoplasm found with the stores in the reservoirs.

In dealing with the materials which constitute the reserves of plant food we must therefore discriminate between these two descriptions of substances. Generally we find a distinction possible in the degree of solubility which attaches to them. The carbohydrates of the reservoirs are mainly starch or inulin ; those of the circulating sap some form of sugar, or vegetable acids derived therefrom. The nitrogenous bodies of the seed are usually either definite grains or collections of amorphous proteids ; those of the sap are mainly diffusible proteids or amide bodies largely derived from them.

In setting out to discuss the various forms of reserve materials, their modes of disposition and their fate, this must be borne in mind and the relations of the two classes recognised.

The kinds of material found in plants which belong to one or other of these categories are extremely numerous and varied. Their places of temporary deposit are very widespread, some charged with great abundance of nutritive substance and evidently set apart for a prolonged resting period ; others containing a little only and this intended for only a short stay in its place of deposition. In seeds, tubers, bulbs, corms, fleshy roots and stems we recognise an almost permanent deposit ; in medullary rays and cells of vascular tissue, in sieve tubes and laticiferous vessels, we find material laid down for only brief periods, ready for an almost immediate demand on the part of the living cells. In these cases we find each to have its most appropriate form, the temporary stores being as a rule of a more easily transformed character than the more permanent ones.

The nitrogenous reserve materials, in whichever of the two descriptions of reservoir we find them, are largely met with in the shape of some form or other of proteid matter. Till within recent years much obscurity existed as to vegetable proteids, many bodies with very curious reactions being

described by various writers, apparently showing no similarity to those of the animal organism. *Legumin*, *conglutin* and other names were applied to them, and their ultimate analyses showed but little agreement. The application of the various methods of examination which threw so much light upon the proteids of the animal body has, however, established the fact that they are of the same nature and of pretty nearly the same composition as the latter, the discrepancies at first so prominent being ascertained to be due to the modes of preparation or extraction.

Besides these proteid bodies, however, other nitrogenous compounds have been identified, chiefly, though not entirely, confined to the fluid sap which is in the living cells. These are of a much simpler composition ; they include asparagin, glutamin, leucin, tyrosin, and other crystalline bodies which from their chemical constitution are grouped together under the name of *amides*. In their origin they are in many cases to be regarded as derived from the more stable proteids, and are indeed the chief form in which the latter are transported from place to place. They have, however, another possible source, being in many cases the expression of an incomplete constructive process and capable of conversion into the proteids themselves, from which later they are again reconstructed.

Some amount of nitrogenous reserve materials may also be found in some of the glucosides, such as amygdalin, present in the seeds and other parts of certain of the Rosaceæ, and myrosin, so prominent in many of the Cruciferæ.

Of non-nitrogenous reserve material we find the greatest amount to be carbohydrate. Here we find starch and its derivatives, cellulose, various kinds of sugar, inulin, and glycogen, giving a great variety of forms and great differences in amount in various plants. Next in importance come the glucosides, less uniformly distributed but still occurring very widely in particular groups of plants. Fats and oils are of common occurrence in many fruits and seeds, sometimes found alone, sometimes with carbohydrates in the same cells.

Besides these substances, of the meaning and import of

which there can be no doubt, we find many others the function of which is still a matter of controversy. Such bodies as tannin and phloroglucin are of fairly widespread occurrence and much is to be said both for and against their nutritive value. The vast number of alkaloidal matters offer a further opportunity for discussion, some facts in their history appearing to point to their possessing, at any rate, some importance in nutrition in particular cases. Lastly, we find in many plants small accumulations of mineral matter often associated with proteid deposits, though sometimes occurring separately, on which still much remains to be said.

In discussing the meaning of these various bodies, the peculiar relation of proteid substances to living protoplasm indicates their primary importance and they necessarily claim first attention. They are easily isolated, occurring as they do in large quantities in many seeds; and from their being found generally without a very large admixture of other bodies they offer a fairly easy task to the analyst. In most works on vegetable physiology they are frequently spoken of in a general way under the name of *aleurone*, or aleurone grains. A section of a seed containing them shows them usually as rounded or oval bodies, sometimes presenting no particular structure, sometimes as containing crystalloid bodies and generally a small aggregation of mineral matter, which is usually a complex phosphate. Occasionally, as in the potato, they have the appearance of a cubical crystal, when the term aleurone grain is abandoned for *crystalloid*. Their original appearance in the cells in the seed has been investigated by several authors. Pfeffer (1) published in 1872 the first observations on record on this point. In his paper he attributes their first formation to the influence of the mineral constituent with which they are associated. The latter, usually either minute crystals of oxalate of calcium, or amorphous collections of the double phosphate of calcium and magnesium, can first be detected in the cell sap of the vacuole of the cell, and some peculiar attraction causes the proteid matter to accumulate round them and enclose them in its substance. As the seed

ripens the sap becomes less and less watery, and gradually the proteid is deposited by a process of precipitation till the complete aleurone grain is formed.

The mode of formation thus indicated seems to show that the process is altogether independent of the presence, or at least the activity, of the protoplasm of the cell. So important a matter as the storage of this form of reserve seems hardly likely to be altogether without the control of the living substance, particularly in the light of Schimper's observations on the mode of formation of starch grains in similar situations. Some careful researches carried out much later by Rendle (2) point to an altogether different mode of formation, at any rate in leguminous seeds, in which aleurone grains are extremely conspicuous. The material used by the latter observer was the seeds of species of *Lupinus*, his chief research being upon *L. polyphyllus*. The early stages of construction of the grains were only observable when the development of the cotyledons was sufficiently advanced to swell out the seed coat, by which time the endosperm had been absorbed. At this time the peripheral layer of protoplasm contained chlorophyll grains, in the substance of which starch was fast accumulating. While the store of carbohydrate food was thus being packed away the aleurone grains gradually became observable. Rendle lays stress on the fact that they first appeared as, or were preceded by, small bodies of spherical or ovoid shape which projected from the protoplasm. In some cells the latter only existed as a peripheral layer, in others it appeared also as bridles crossing the vacuole. Wherever the seat of formation could be noticed its first sign was this protoplasmic protrusion. Little by little these projections increased in size, growing inwards as well as outwards, till the grains could be seen as ovoid or spherical bodies embedded in the protoplasm, which, in consequence of their development, assumed the appearance of a coarse network. Gradually, as the process thus went on, the older grains increasing in size and newer ones being subsequently formed, the original vacuole became obliterated and the cell was swollen out by its own deposits. This mechanical process was accompanied

by a gradual change of a chemical nature. At first the deposit or secretion stained homogeneously much more deeply than the protoplasm; the grains dissolved in dilute caustic potash, but not completely, a membrane always being left. At this early stage they were not dissolved by sodium chloride either in ten per cent. or saturated solution, though they were soluble in these fluids when mature. As they advanced in development they became vacuolated, remaining always denser on their periphery. Finally they were again homogeneous and then showed the reactions of the grains of the ripe seed. Intermediate conditions could be observed as maturity was gradually approached.

Rendle thus argues that the process of aleurone grain formation is one of true secretion and not of a mere mechanical precipitation. He opposes Pfeffer's view of the part played by the mineral matter, observing that in *Lupinus* at any rate the cells contain no crystalline bodies at all at the stage when the grains begin to be formed.

Another view of the mode of their formation has been advanced. Wakker writing in 1888 (3) describes what he believes to take place in the endosperm of the castor oil plant (*Ricinus communis*). He notes that the protoplasm of the cells is greatly vacuolated, and suggests from his observations that each vacuole corresponds to an aleurone grain. This vacuole becomes filled little by little with the albuminoid matter as the seed ripens. Werminski (4), working on the same seed, in the main confirms his observations and supports his views. Van Tiegham has suggested from the firmness of the protoplasm round the vacuoles that the latter are not mere cavities in the living substance, but that they really correspond to plastids, and as they are filled generally with sap only they may be called *hydroplasts*. As the aleurone grains, according to Wakker's observations, are always formed in these vacuoles, we have a kind of plastid formation, something like the work of the amyloplasts.

This view has been criticised adversely by Ludtke (5), who in the main agrees with Rendle in regarding the process rather as one of intraprotoplasmic secretion. His

observations were made upon *Ricinus communis* and *Linum usitatissimum.* According to him both crystalloid and globoid originate in the protoplasm and not in the vacuole. He denies that the vacuole has any definite limiting membrane or that it has any particular value in the process. Sometimes he was able to observe a little space between the newly-formed grain and the neighbouring protoplasm, but nothing more definite than this. As the globoid, according to him, is very early to be observed, he goes back to a certain extent to the views of Pfeffer.

The secretion theory is opposed by Belzung (6), in a paper published in 1891, in which he supports the view of a mere mechanical precipitation much as Pfeffer did. The formation of the grains begins at a somewhat late period, when the cell sap is becoming more or less concentrated. The proteid or albuminoid principles, which will give rise to the grains, are, he says, partly dissolved in the sap by virtue of the alkaline phosphates which are present in solution and partly combined with soda or potash, in the form, therefore, of albuminates of these metals. Very weak solutions of the alkalis are well known to be appropriate solvents of these proteids. Weak acids precipitate the proteids from such combinations in the form of a whitish powder insoluble in water. At this point in the development of the seeds such weak acids, particularly citric acid, are present in the cells and their action causes a gradual precipitation, which proceeds until the aggregation of the precipitate can be seen to take the form of the completed grains. The progressive loss of water as the seed goes on to maturity causes the precipitation to be more abundant, so that the grains grow rapidly. Thus the formation is purely physical.

He considers his ideas confirmed by the ease with which artificial grains can be produced from solution of the proteids. Werminski obtained them thus from the material of *Ricinus*, and Bredow from that of *Lupinus luteus* by a process of dehydration.

Belzung's description of the way in which the aleurone grains first appear in the cells coincides fairly well with

the observations of Rendle, and seems rather opposed to his theory of their mechanical deposition. The formation begins late while the cells are a little short of their mature condition and the grains appear first at the periphery of the cells against the cell walls ; that is, in the limiting layer of the protoplasm, and not, as a mechanical hypothesis would suggest, in the vacuole of the cell. After their first appearance they grow rapidly, and vacuoles form in their interior. The grains so appear to have a definite structure, consisting of a firm framework, enclosing within its meshes a peculiar sap, rich in various constituents of which proteid matter forms the chief part. The wall, or meshwork, is of different composition, being insoluble in water. The sap in its meshes is replaced as the seed ripens by proteid matter alone, except that organic acids, galactine, etc., may be detected in the latter in small quantity.

Belzung's work was carried out on leguminous seeds, chiefly the haricot bean, the common bean, the pea, the lupin and the broom.

In weighing the value of these several hypotheses as to aleurone grain formation and attempting to decide whether it is a vital process or no, it is clear that we have to take into account the fact that there is a copious accumulation of proteid matter in the cells as the seed is maturing. The appearance of the grains can be explained in two ways. Either they are formed in consequence of the transport of proteid in solution to the cells in such a condition as to be readily precipitated by the organic acids, etc., formed there, or they must be constructed from other plastic materials at the place where they are deposited. If they are transported in proteid form they must travel either as peptone or in the condition in which Belzung suggests they exist in the cell antecedently to their precipitation, that is, either as albuminates of an alkali metal, or in some state of solution with the phosphates in the sap. It is difficult to entertain either view ; peptone is the only form of proteid which is at all easily diffusible through membranes, such as cell walls ; but peptone has not been found in the cells during or before the deposition of the grains, nor would peptone be trans-

formed by weak organic acids into such proteids as are found in the aleurone. On the other hand, even if weak acids in the cell could cause a precipitation from the solution of the proteids in the second suggested condition, it is extremely unlikely that such solutions could pass readily from cell to cell, as must be the case for the accumulation of the aleurone grains. The hypothesis of protoplasmic secretion has much more to be said in its favour. It agrees with the way in which carbohydrate formation takes place, the latter being always the result of the work of a plastid or of protoplasm itself, as we shall see later. It explains also the large amount deposited, as the constructive activity of the living substance is at the time considerable.

Taking this view there are two modes again of regarding the process. The living substance may deposit it as a definite secretion from its own substance just as the protoplasm of other cells may deposit enzymes and other substances, or it may transform into proteid bodies other materials sent to the cells which, though not proteid, nevertheless are substances on the way to become so. That is, that the process of proteid construction, normally occurring in the leaves, may also take place in the reservoirs just as in the case of carbohydrates. This is a probable view, and is supported by the fact that when aleurone grain formation is beginning, amide bodies, and particularly asparagin and leucin and sometimes tyrosin, can be shown to be present in the cells. We know from other experiments that when the aleurone is being used in constructive processes during the germination of the seed such amides do appear as the result of the decompositions accompanying germination. Indeed, these bodies appear from many considerations to be the form in which the transport of nitrogenous nutritive material takes place in the plant. Their occurrence in the cells, therefore, while deposition of aleurone is taking place, indicates them as a probable immediate antecedent of the grains themselves. If this is so, then we must take the view of protoplasmic construction and not mechanical deposition, as we are not able, apart from

the influence of living substance, to convert amides into proteids.

We may now examine the aleurone grains with a view to ascertaining what proteids they are composed of. These are found to vary in different seeds, but the variety is not very great. The classification of them turns upon their solubility in various solutions or extracting fluids. On this point most of our information is derived from the investigations of Weyl (7) and of Vines (8). The former observer experimented on the seeds of oats, maize, peas, almonds and mustard, and on Brazil nuts. He extracted his seeds first with water and subsequently with solutions of neutral salts of various strengths. His watery extracts contained certain bodies which have since been shown to belong to the group of the albumoses; his salt solutions extracted two globulin bodies showing different properties. The strength of solution of most value was 10 per cent. and the salt was sodium chloride. The extract of the seeds prepared with this solvent was found to give a copious precipitate or coagulum on boiling. Further experiments showed that it contained two globulins, one of which was thrown down by saturating the solution at the ordinary temperature with crystals of common salt while the other remained dissolved in the concentrated solution. On heating solutions of these two proteids separately they were found to coagulate at different temperatures, the first at 55° to 60°C., the other at 73°C. Weyl gave the name of vegetable myosin to the former and that of vegetable vitellin to the latter.

The work of Vines carried the matter further. Besides working on quantities of the seeds and extracting the proteids on a large scale he examined the action of the solvent fluids upon sections of the seeds under the microscope. His most interesting results were obtained with the grains of the castor oil seed and those of the Brazil nut. In these cases the grains are of complex composition, showing a well-defined crystalloid and globoid in the interior of each. When a section of the endosperm of the castor oil bean is examined, after removal by alcohol of the oil it contains the

aleurone grains are seen as opaque oval bodies lying in the dense protoplasm. Water passed through the preparation dissolves a portion of the exterior, making them transparent and bringing to view the crystalloid and globoid in their interior. Lavage of the section with 10 per cent. solution of NaCl dissolves the rest of the grain, leaving the crystalloid and globoid free. Saturated salt solution then dissolves the crystalloid. The globoid, which is mineral in character, being composed of the double phosphate of calcium and magnesium, remains alone in the cavity where the original aleurone grain had been.

Vines examined a very large number of seeds by this method and in the main confirmed Weyl as to the composition of the aleurone grains. In some seeds, particularly in those of *Sparganium racemosum*, he discovered another proteid belonging to the class of albuminates or derived albumins, soluble only in dilute acids or alkalis. This proteid he found to occupy the external portion of the aleurone grain.

We have thus in these seeds two members of the globulin family differing in their degree of solubility in neutral salt solutions and in the temperature at which they are changed by heating. The proteid soluble in water was called vegetable *hemialbumose;* it has since been shown to be a mixture consisting chiefly of protalbumose and hetero-albumose. The characteristic reaction which is given by both these bodies and hence by Vines' watery extract is that the proteid is precipitated by nitric acid and that the precipitate is dissolved by warming the liquid, and thrown down again as the solution cools.

Following on the investigations of Vines, but some few years later, we have the results of Martin's examination of the seeds of *Abrus precatorius* (9), the jequirity or Indian liquorice, which are toxic in their properties. The proteids were extracted from the ground seeds by soaking the powder for twenty-four hours with 15 per cent. solution of sodium chloride, which was then filtered off. The extract was saturated with ammonium sulphate, which precipitated them. They were found to be two in number, a globulin

and an albumose. The former is readily soluble in 10 to 15 per cent. sodium chloride solutions and coagulates on heating to about 70°C. The albumose is nearly identical with Kuhne's deutero-albumose and appears to be the same body as the same observer discovered to exist in the latex of the Papau (*Carica papaya*). It will be described later.

As already mentioned, besides aleurone grains, proteid has been found in definite bodies in the cells of the tuber of the potato, where it exists in the shape of crystals of cubical form. These are insoluble in 10 per cent. solution of sodium chloride, but dissolve at once in saturated solutions. They belong, therefore, to Weyl's vegetable vitellin. Zöller has found (10) in the potato also some vegetable myosin, which is soluble in 10 per cent. sodic chloride solution. This is probably in the form of amorphous proteid in the protoplasm of the cells, as the microscope does not reveal its presence. Other observers have pursued the same line of investigation, and with the better means of examination that have been devised in recent years have made still further determinations of the nature of the proteids of the seed. Chittenden and Osborne (11) in 1891 discovered that the maize contains more than one globulin and a mixture of albumins, the latter being soluble in distilled water, and the solution coagulating on boiling, therein showing a difference from the albumoses, which are not so changed. Besides these they describe a peculiar proteid which differs from all others so far found by being insoluble in water, but soluble on warming with dilute alcohol. To this they have given the name of *zein*. The action of alcohol on the grains was shown by Vines to be somewhat peculiar, many of the crystalloids being insoluble in saturated solution of sodium chloride until after treatment with alcohol, when they dissolved readily.

The occurrence of proteid in some such forms as some of these was indicated by Zacharias (12) in sieve tubes, where it became coagulated on heating. Fischer (13) made a similar discovery in Cucurbita. Zacharias describes his proteid as insoluble in water or neutral salts,

but soluble in dilute acids and alkalis. It is coagulated by contact with alcohol and gives the usual chemical reactions.

Besides the stores of proteids thus evidently reserve materials deposited for use after a considerable resting period, we find other stores more immediately available for nutritive purposes. In many plants definite channels in the tissue are stored with a fluid of considerable complexity, known generally as latex. Probably this fluid contains many bodies which are not to be regarded as constructive materials, but rather as in some sense waste products, or at any rate as bye-products of metabolism. Besides these, however, are many plastic substances, such as starch, sugar, etc., and most latex contains considerable quantities of proteids.

Martin (14) has investigated with great care the latex of the Papau (*Carica papaya*), working, however, on the dried juice. He has proved that this fluid contains two well-defined albumoses, a globulin and an albumin. To them he attributes the following reactions:—

Globulin.—Soluble only in solutions of neutral salts, and precipitated from such solutions by saturation with either sodium chloride or magnesium sulphate.

Precipitated from its solutions by large dilution followed by the passing of a stream of CO_2 through the dilute extract for some time.

Precipitated by dialysing till the neutral salts are removed.

Coagulated on heating the solution in 10 per cent. sodium chloride to 70 to 74°C.

This globulin resembles Weyl's vegetable myosin in most respects, but its coagulation temperature is considerably higher.

Albumin.—Soluble in water and not precipitated by saturation with neutral salts. Coagulated in heating and on addition of nitric acid.

αPhytalbumose. — Soluble in cold or boiling water, not precipitated from neutral solution by NaCl or $MgSO_4$ but thrown down after long shaking in slightly acid solution. Martin considers this body to be much like Kuhne's protalbumose (15). It is probably a constituent of Vines' hemialbumose.

βPhytalbumose.—Soluble in cold water; precipitated by heat in two stages, the first precipitate falling at 78° to 82°C. and the second at 83° to 95°C.

Not precipitated by dialysis.

Both these albumoses give the characteristic behaviour of their nitric acid precipitate.

Martin points out the resemblance between the latter body and Kuhne's hetero-albumose.

In 1885 the writer (16) had the opportunity of examining the latex of several trees, particularly *Mimusops globosa*, *Parameria glandulifera* and *Brosimum galactodendron*. *Mimusops globosa* is the source of the gum balata of British Guiana. Its latex contains an albumose which is soluble in distilled water, and is not coagulated by boiling in neutral solution. At a temperature approaching 70°C. it is slowly thrown down by nitric acid. It is not precipitated by acetic acid and potassic ferrocyanide.

Parameria is a member of the Sapotaceæ. Its latex contains a body more like a peptone than any of the foregoing, being capable of dialysis. It does not, however, give easily the biuret reaction so characteristic of the latter group. It is convertible into peptone by the action of pepsin.

Brosimum galactodendron is the so-called "cow-tree" of South America and is a member of the Artocarpeæ. It has been known to yield a rich milky latex since 1823, when it was the subject of a paper by Boussingault and Mariano de Rivero, in which attention was called to its containing what the authors regarded as vegetable fibrin. The chief proteid present in the latex is an albumin, which is soluble in water and coagulates on heating to 68°C. or by treatment with alcohol. After removal of this body the latex

is found to yield the same peptone-like body found in Parameria.

Besides the laticiferous tissue we find other parts of plants, especially the succulent cortex of stems, the parenchyma of fleshy roots, etc., the seat of proteid stores. These are not necessarily reserve materials, being possibly plastic material on its way to be used up in constructive processes. In the large fleshy roots of biennial plants and possibly also in succulent perennial roots the contents of the cells must however be regarded as true reserves.

In the cortex of the stem of the Manihot there is to be found a globulin a good deal like those already described. It is insoluble in water but soluble in solutions of neutral salts. From the latter it can be precipitated by dialysis, by large dilution or by saturation with solid $MgSO_4$. Its coagulation temperature in neutral solutions is 74° to 76°C.

In the succulent tissue of the lettuce (*Lactuca sativa*) there is an albumose which is soluble in distilled water and is not coagulated on boiling, but is precipitated by nitric acid or by acetic acid in the presence of potassic ferrocyanide.

But little work has been done upon fleshy roots, but an investigation made recently on the asparagus (17) shows in the underground parts of this plant an albumin which gives the characteristic reactions of that group.

The proteids found in latex and in the various succulent tissues described are not in the forms of the aleurone grains of seeds. They are either in solution in the complex sap of the cells of the tissue, or are packed away in amorphous form in the meshes of the protoplasm. If cells of these parts are treated with alcohol under the microscope the clear, transparent vacuoles become turbid and opaque, indicating a precipitation of the proteid by the spirit.

The list of proteids so far discovered may be supplemented by true peptone, which can be determined in germinating seeds both of leguminous plants (18) and of cereals.

This peptone is probably, however, but a stage in the transformation of the resting reserve material to amide bodies which are transported from the food reservoirs to the seats of active growth.

So far, then, the reserve proteids proper are non-diffusible, granular or amorphous bodies, with difficulty soluble and almost without power of dialysis. They include several forms which show differences in these respects. There are several albumoses, soluble much more readily than the other kinds and apparently nearest in constitution to the diffusible peptones; globulins, comprising members of two groups which show great differences in their behaviour to heat, the one coagulating between 50° and 60°C., the other between 70° and 80°; and albumins. In regions where storage is not so evident we have in addition members of the peptone group, giving characteristic reactions and capable of dialysis.

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(To be continued.)

INDIAN GEOLOGY.[1]

IT is the misfortune of the stratigraphical geologist that the interest which is taken in his work varies inversely as the distance of the scene of his labours. Indian geology therefore stands but a poor chance of recognition in England; and the Indian geologist has only the melancholy satisfaction of knowing that he writes for posterity.

This ought not to be. There is much in the geology of India which can never be of interest outside of India itself; but there is much also which bears on the widest and greatest questions of the science. It is in India and in the southern hemisphere that we find the clearest evidence of a period of severe cold towards the close of the palæozoic era—evidence which only those who have not seen it have dared to doubt. It is in India and on the frontiers of India that we have the grandest mountain-chains of the globe, where the problems of mountain-building may frequently be studied without the vexatious hindrance of vegetative growth. In India, too, there are the great lava-flows of the Deccan Trap, and the magnificent mass of ancient crystalline rocks in Southern India;—but the geologist does not boast of these, for they have never been adequately examined.

The only general account of the geology of India which we had until lately was the *Manual* by Medlicott and Blanford, published in 1879. But Indian geology has not stood still since then, and the new edition of this *Manual* (edited and largely rewritten by R. D. Oldham) which has recently appeared shows how great is the progress that has been made. It is no bad record for the labours of a dozen men who are scattered over the whole of India and the adjoining countries, and who are now required by a short-sighted Government to devote the greater part of

[1] *A Manual of the Geology of India.* Chiefly compiled from the observations of the Geological Survey. *Stratigraphical and Structural Geology.* Second edition, revised and largely rewritten by R. D. Oldham. Calcutta, 1893.

their energies to inquiries of a purely economic nature. But it is deeply to be regretted that palæontological research has not kept pace with stratigraphical, for the work of the survey would be of infinitely more value to other geologists if the fossils collected were always properly compared and described.

When the first edition of the *Manual* was published, our knowledge was so fragmentary that it was impossible to give a connected account of the geology of India as a whole; and it was found necessary to break up the book into a series of descriptions of different districts. But so great has been the progress made since that time, that Oldham has been able to adopt a chronological arrangement; and although in many cases the chronology is still very doubtful, this plan greatly facilitates the comprehension of the work.

In this article it is my object to draw attention to some of the more important of the recent advances in Indian geology. Fuller information on all the points raised will be found in Oldham's *Manual*, except in the case of one or two observations which have appeared since the publication of that work; and references to the more important of the papers dealing with the questions discussed are given in the footnotes.

Few parts of the world can boast a finer development of "gneissic rocks" than Southern India; and no more promising field can be found for their investigation. But unfortunately it was in the early days of the Geological Survey, when petrology was still in its infancy, that this part of India was chiefly studied; and at that time it was almost universally held that gneiss had been formed by the metamorphism of ordinary sedimentary strata. Little attention was paid to these rocks, and on the map accompanying the first edition of the *Manual* they were all grouped under one colour.

Foote was the first to show that it was possible to distinguish a more schistose series, consisting of conglomerates, schists, hæmatite-beds and lava-flows, from the more truly

gneissic rocks ; and to this series he gave the name of "Dharwar".[1] It forms long bands, or rows of elliptical patches, which run from N.N.W. to S.S.E. ; and economically it is of great importance, for all the quartz reefs which bear gold in paying quantities occur within it. Since quartz reefs are equally abundant in the gneiss, it is most natural to suppose that the gold lay originally in the Dharwar rocks and has since collected by some process of "lateral secretion" in the reefs which now traverse them. And since the Dharwars consist largely of lava-flows, we may presume that the gold was brought up from below in those flows.

The Dharwar rocks invariably overlie the gneiss and are evidently newer than the greater part of it ; but the relations of the two are not quite simple. Often near their borders the Dharwar bands are penetrated by masses and veins of gneissic rock ; and this alone is sufficient to suggest that some parts of the gneissic system are eruptive and newer than the rest. Subsequent researches have tended to confirm this view, and the matter awaits further investigation.[2] It is unfortunate that there are no detailed petrological observations on the gneiss of India except those of Lacroix,[3] which were based on collections made many years ago in the Salem district. One of the most remarkable peculiarities, which has been noticed by several observers in various parts of India, is the abundance of hypersthene in these rocks.

In the Peninsula the gneisses and Dharwars are succeeded by a great thickness of unfossiliferous rocks, and no remains of life appear in any beds older than the Gondwanas (Carboniferous-Jurassic), but in the Punjab an un-

[1] See especially R. B. Foote. "The Dharwar System, the chief auriferous rock series in South India". *Rec. Geol. Surv. India*, vol. xxi. (1888), p. 40 ; vol. xxii. (1889), p. 17.

[2] Referred to in *Rec. Geol. Surv. India*, xxvi. (1893), p. 172 ; xxvii. (1894), p. 7.

[3] "Contributions à l'étude des gneiss à pyroxène et des roches à wernérite." *Bull. Soc. Franc. Min.*, 1889. Translated, so far as it relates to India, in *Rec. Geol. Surv. India*, xxiv. (1891), p. 155.

doubted Cambrian fauna has recently been discovered in the rocks of the Salt Range.[1] The most interesting forms are trilobites, of which two have been described under the names *Conocephalites Warthi* and *Olenus indicus;* but the reference of the latter to the genus *Olenus* is very doubtful. According to most observers the "Salt Marl" is older than the beds in which these fossils occur, but Middlemiss has attempted to show that it is not a sedimentary deposit but is of hypogene origin and has been intruded into its present position.[2] The proof of this, however, is not complete.

Beds which are referred to the Cambrian and Silurian are also found in the Himalayas;[3] but the fossils which occur in them have not been determined. Near Mandalay also, Silurian beds with *Echinosphærites* are stated to be present;[4] but this requires further confirmation.

It is not till we reach the Carboniferous that we find beds in which fossils are at all widely spread. In the Peninsula the fossiliferous part of the geological sequence commences with the great Gondwana system, the deposition of which appears to have begun towards the close of the Carboniferous period. It is divided into two groups,—the lower characterised by Equisetaceæ and ferns of the *Glossopteris* type; and the upper by Cycadaceæ.

At the time of the publication of the first edition of the *Manual* the age of these beds was one of the burning questions of Indian geology.[5] The difficulty of correlation

[1] King. "Note on the discovery of Trilobites by Dr. H. Warth in the Neobolus beds of the Salt Range." *Rec. Geol. Surv. India*, xxii. (1889), p. 153. Waagen. "Salt Range Fossils." *Pal. Ind.*, ser. B, vol. iv., pp. 92, 104.

[2] Middlemiss. "Notes on the Geology of the Salt Range of the Punjab, with a re-considered theory of the origin and age of the Salt Marl." *Rec. Geol. Surv. India*, xxiv. (1891), p. 19.

[3] Griesbach. "Geology of the Central Himalayas." *Mem. Geol. Surv. India*, xxiii. (1891).

[4] Noetling. "Field notes from the Shan Hills (Upper Burma)." *Rec. Geol. Surv. India*, xxiii. (1890), p. 78. See also Noetling. *Ibid.*, xxiv (1891), p. 104.

[5] The literature of the subject is extensive but has lost its former interest, and no reference is needed but to Oldham's admirable summary in the *Manual*, second edition, chap. viii.

arises from the fact that the Gondwana flora and fauna consist almost entirely of land and freshwater forms, very few of which have ever lived in Europe; and it is only indirectly that it has been possible to compare the Indian with the European sequence. Fortunately, in Australia, beds with plant-remains closely resembling those of India are associated with others which contain a marine fauna; and these marine beds can be correlated with the Carboniferous of Europe.

But this is not all. The Talchir group at the base of the Gondwana system in India, consists largely of a fine silt or clay; and scattered irregularly through the clay lie numbers of boulders of all sizes. In many cases the boulders must have been brought from a distance; for the rock of which they are made, is unknown in the neighbourhood. Occasionally too they are scratched and striated;[1] and the deposit was evidently ice-borne.

Similarly in New South Wales the marine Carboniferous beds consist of fine sand or shale in which are embedded subangular blocks of all sizes, some of which are striated. Intercalated among these beds are the Lower Coal Measures of Stony Creek, which contain *Glossopteris* and other Lower Gondwana forms; and above them come the Newcastle Coal Measures, the plants of which are for the most part identical with those of the Damudas, which in India succeed the boulder-bearing beds.

In the face of such evidence as this, it is impossible to doubt that the Indian and Australian beds were contemporaneous. The Australian beds can be correlated by their marine fauna with the Carboniferous of Europe; and the conclusion is irresistible that the deposition of the Gondwana beds of India began at about that period, and this conclusion is in harmony, as will appear presently, with observations which have been made in the Salt Range.

With regard to the upper limit of the Gondwana period, the evidence is not so clear. In the Umia group of Cutch,

[1] Fedden. "On the evidences of 'ground-ice' in tropical India, during the Talchir period." *Rec. Geol. Surv. India*, viii. (1875), p. 16.

plant-bearing beds are associated with the normal marine deposits of the group, and among the plants are several which are found elsewhere in the highest beds of the Gondwana system. Hence we may conclude that the Umia beds are approximately on the same horizon as the uppermost Gondwanas. Now the Umia plant-beds overlie beds which contain *Ammonites tomephorus*, *A. eudichotomus* (Tithonian forms), and *A. suprajurensis* and *A. bleideri* (Portlandian forms). Evidently therefore they belong to the very end of the Jurassic period; and the Gondwana system came to a close at about the same time. This being so, it is remarkable that many of the plants of the highest Gondwanas are said to be identical with, or closely to resemble, forms from the Lower Oolite of England. It is greatly to be regretted that the marine fossils associated with the Gondwanas of the east coast have not been more closely examined.

Turning now to the north, the Silurian beds of the Salt Range are overlaid unconformably by a group of sandstones known as the "Speckled Sandstone," at the base of which there is almost always a boulder-bed with striated boulders of rock which have come from a distance. The analogy with the Talchir deposit naturally suggests itself at once. No plants have been found; but in the beds immediately overlying the boulder-bed, *Conularia* and other marine fossils occur.[1] Thirteen out of the twenty-two species are identical with forms from the marine Carboniferous of New South Wales, which is also associated with a boulder-bed (already correlated with the Talchir boulder-bed). Only the most confirmed sceptic—with views of his own—can doubt that the boulder-beds of the Salt Range, of the Talchirs, and of New South Wales are on the same horizon; and since a similar bed, also associated with a *Glossopteris*

[1] Waagen. "Note on some Palæozoic Fossils recently collected by Dr. H. Warth in the Olive Group of the Salt Range." *Rec. Geol. Surv. India*, xix. (1886), p. 22. It was at one time doubted whether these fossils were contemporaneous with the rocks in which they occur; but this doubt has vanished in the face of further evidence.

flora, is found in South Africa, it is difficult to resist the belief that around the Indian Ocean at least, there was a period of severe cold towards the close of the Palæozoic era.[1]

The speckled sandstone of the Salt Range is succeeded by a great thickness of deposit, mostly calcareous, which contains one of the most interesting faunas of India. According to its fossils the upper part of the speckled sandstone itself must be included in this series. It is the Productus Limestone group[2] of Waagen, and, with his limitations, not a single species passes from it either into the beds below or into the beds above.

As already remarked, the lower part of the speckled sandstone contains fossils which are for the most part identical with those of the marine Carboniferous of New South Wales. But not one of these passes up into the beds above; and the fauna of the Productus group shows European rather than Australian affinities. It would lead us too far to attempt the correlation of the various subdivisions of the group, and it must suffice to say that the lower beds appear to be on the horizon of the upper Carboniferous or of the Permo-Carboniferous of Russian geologists (Artinsk stage of the Urals, etc.); while the upper beds are probably a little newer than the Permian. The most interesting feature of the whole group is the presence of true ammonites along with Palæozoic brachiopods.

The Productus Limestone group is succeeded by beds with Ceratites, and lithologically these might well belong to the same group. Indeed, originally the two were not distinguished, and the collections which have been made from the Ceratite beds since they were separated, have not yet been examined. A preliminary note upon them, however, has recently been published by Waagen,[3] from which

[1] Waagen. "Die Carbone Eiszeit." *Jahb. d. k. k. Geol. Reichs.*, xxxvii. (1887), 143. Translated in *Rec. Geol. Surv. India*, xxi. (1888), p 89. A similar boulder-bed is reported in South America, but on extremely imperfect evidence.

[2] "Salt Range Fossils." *Pal. Ind.*, ser. 13.

[3] *Rec. Geol. Surv. India*, xxv. (1892), p. 182.

it appears that the Ceratite beds probably include representatives of the Bunter, Muschelkalk and Keuper. Cephalopods are very abundant in the Bunter (where in Europe they are very rare); but are much less common in the upper beds. It is remarkable that, whereas ammonoids with true ammonitic sutures occur in the Productus Limestone, all the ammonoids from these Ceratite beds show ceratitic, or rarely goniatitic, sutures. In the Himalayan Trias, on the other hand, ammonoids with ammonitic sutures are common in the Trias.

In the Central Himalayas [1] the unconformity which is found in the Salt Range between the Lower and Upper Palæozoics is absent; and both Devonian and Carboniferous rocks have been described. But their fossils are yet unexamined.

The Carboniferous is overlaid—it is believed unconformably—by beds which are regarded as Permian and Trias; and of these the Trias has yielded an important series of fossils, which is now undergoing examination. The whole of the Triassic sequence appears to be present,[2] and the lower part, as in the Salt Range, is characterised by a great abundance of cephalopods. The higher beds show gradually increasing European affinities, until in the Rhætic portion of the series the deposits are said to be almost indistinguishable, both lithologically and palæontologically, from those of the Alps.

Finally before leaving the Upper Palæozoic and Lower Mesozoic rocks (which in India are naturally grouped together) it may be noted that Carboniferous fossils occur in Tenasserim, and have recently been described by Noetling.[3]

The Cretaceous rocks of India are not very extensive, but they are of great importance, for it is on them that

[1] Griesbach. "Geology of the Central Himalayas." *Mem. Geol. Surv. India*, xxiii. (1891).

[2] See a note by Mojsisorics, *Sitz. d. k. Akad. Wiss.*, Vienna, Math. Nat. Class. ci. (1892), translated in *Rec. Geol. Surv. India*, xxv. (1892), p. 186

[3] *Rec. Geol. Surv. India*, xxvi. (1893), p. 96.

Neumayr and others have based their view that Africa and India were at one time united by a continent which stretched across the Indian Ocean. The facts on which they rely are briefly these: The Cretaceous fauna of the north-west of the Indian Peninsula is very different from that of the south and east. The former is allied to the Cretaceous fauna of Europe; the latter closely resembles that of South Africa.

On the western side of India the chief area of Cretaceous rocks occurs in the valley of the Narbada near the turn of Bágh. Fossils are not very abundant, but they are of great interest. An attempt has been made[1] to show that the whole of the European Cretaceous from the albian to the senonian is represented; but the palæontological evidence is entirely insufficient to support this idea, and Oldham[2] returns the older view that the Bágh beds are all of cenomanian age. By far the greater number of forms which have been determined with certainty occur also in Europe.

On the east coast of India matters are entirely different. In the neighbourhood of Trichinopoly, Pondicherry and Viruddhachalam three distinct horizons are recognisable.[3] The lowest contains such typical cenomanian forms as *Ammonites rotomagensis*; the middle includes the turonian forms *Am. peramplus*, etc.; and the upper beds contain *Nautilus danicus*, *Inoceramus Cripsi*, *Crania ignabergensis*, which are characteristic of the senonian or danian. Thus the correspondence with the European sequence is fairly close; but in spite of this the greater number of the forms are absolutely unknown in Europe. Indeed out of a total of nearly 800 species of invertebrates found in South India, less than one-sixth occur in any European country. Simi-

[1] Bose. "Geology of the Lower Narbada Valley between Nimáwar and Káwant." *Mem. Geol. Surv. India*, xxi., pt. i. (1884).

[2] *Manual*, second edition, p. 251.

[3] See H. F. Blanford, "On the Cretaceous and other Rocks of the South Arcot and Trichinopoly districts, Madras," *Mem. Geol. Surv. India*, iv., pt. i. (1862); Stoliczka and Blanford, "The Cretaceous fauna of Southern India," *Pal. Ind.*

larly, on the most favourable view, out of forty species known in the Narbada area, only one-third are found in South India; and many of these are open to doubt.

On the other hand the Cretaceous of the Khasi hills between Assam and Sylhet is closely related to that of South India;[1] and nearly every species which is known in the former occurs in the latter. Moreover, both contain many South African forms. Out of thirty-five species of mollusca and echinodermata found in certain deposits in Natal, no less than twenty-two are specifically identical with forms of the South Indian Cretaceous.

It is thus clear that the Narbada Cretaceous is much more nearly allied to the Cretaceous of Europe than to that of Assam, South India or South Africa; while the faunas of these three places are closely related to one another. Hence it is concluded that South Africa, the east coast of India, and Assam lay on the south coast, and the Narbada area on the north coast of a tract of land which stretched across the Indian Ocean.

The Cretaceous rocks of Northern India are also interesting in another way. Although they are nearly allied to those of Europe yet they differ from the ordinary European development in this,—that there appears to be a complete passage from the Cretaceous to the Tertiary. In Baluchistan Oldham[2] has described a group of limestones with nummulites; and this group passes laterally into shales which contain Crioceras, Baculites, and Ammonites as well as nummulites. Doubt however has been cast on the details of this observation by Griesbach,[3] and the matter requires further observation. However this may be, the fact of a passage from Cretaceous to Tertiary is well authenticated in Sind.[4] The *Cardita beaumonti* beds contain a fauna

[1] Stoliczka in *Mem. Geol. Surv. India*, vii. (1869), p. 181, etc.

[2] "Report on the Geology of Thal Chotiali and part of the Mari country." *Rec. Geol. Surv. India*, xxv. (1892), p. 18.

[3] "On the Geology of the country between the Chapper Rift and Harnai in Baluchistan." *Ibid.*, xxvi. (1893), p. 113.

[4] W. T. Blanford. "The Geology of Western Sind." *Mem: Geol. Surv. India*, xvii. (1879).

which is neither definitely Mesozoic nor definitely Kainozoic, but is intermediate between the two.

We now come to the last, and perhaps the most important of all the questions which will be referred to here, *viz.*, the origin and age of the Himalayan chain. The subject is fully discussed by Oldham in perhaps the most interesting of all the chapters in the *Manual;* and this, at least, is a chapter which no geologist should omit to read. It is to Medlicott[1] that we owe our first conception of the way in which the Himalayan chain grew; but it was the researches of Middlemiss[2] in Kumaun and Gahrwál which put the matter on a satisfactory basis and laid the real foundations of our knowledge of the process.

From a geological point of view the Himalayas may be divided into three zones which correspond more or less closely with the great orographical features of the range. The first is the Tibetan, which lies to the north of the chain of snowy peaks; and in this belt marine fossiliferous rocks are well developed, but except towards the north-west end of the range they do not extend to the south. The second is the zone of the snowy peaks and of the Lower Himalayas to the south of them; and these are made up of crystalline and metamorphic rocks, and of unfossiliferous sedimentaries (believed to be Palæozoic). And the third is the zone of the Sub-Himalayas or foot-hills, which form the margin of the range towards the Gangetic plain and which consist entirely of Tertiary, and principally of Upper Tertiary, rocks. It is the last zone which has been most carefully examined and concerning which we have the most accurate information. The rocks which compose it are divided into:—

Upper Tertiary or Siwálik,
Lower Tertiary or Sirmur.

The Lower Tertiary commences with a group of clays with some limestones and sandstones, in which marine

[1] *Quart. Journ. Geol. Soc.*, xxiv. (1868), p. 34; and *Manual*, first edition, chaps. xxii., xxiii.

[2] "Physical Geology of the Sub-Himalaya of Gahrwál and Kumaun." *Mem. Geol. Surv. India*, xxiv., pt. ii. (1890).

fossils occur; and this passes up with perfect conformity into the sandstones and clays of the upper part of the Sirmur series. The average coarseness of the deposit gradually increases in the upper beds; and this gradual increase of coarseness is maintained throughout the Upper Tertiary or Siwálik group until we reach the very coarse conglomerates of the Upper Siwálik.

West of the Jehlam the whole series is said to form a perfectly conformable sequence; and many of the sections in Kumaun and Gahrwál show a similar conformable passage from the Lower Tertiary to the Middle Siwáliks and from the Middle to the Upper Siwáliks. But this is not invariably the case; and in many places, especially towards the interior of the chain, the Upper Siwáliks rest upon the upturned and eroded edges of the Lower Siwáliks. Only one explanation is possible, namely, that the series of rocks was deposited during a period of disturbance, and that while continuous deposition went on in one area, in another the beds were raised and denuded and buried again.

Along the whole length of the Himalayan chain, wherever the Siwáliks are in contact with the pre-Tertiary rocks, the line of junction is a great reversed fault. In places this fault lies between the Lower Tertiaries and the older rocks; and in one area it lies between the Lower and Upper Tertiaries. Similar great boundary faults are not uncommon—indeed they are probably almost universal along the margins of great mountain chains;—but the peculiarity[1] of the Himalayan fault is that it is also a boundary of deposition. The Tertiary series never extended much to the north of the fault; for it is impossible to believe that the whole enormous thickness of these beds could have been removed so regularly and so completely as to leave no outliers.

Hence the conclusion is irresistible that during the deposition of the Sub-Himalayan Tertiaries the boundary of the Himalayan Range coincided approximately with this

[1] I am far from believing that this feature is really peculiar to the Himalayan Range; but it is more conspicuous and has been more clearly proved there than perhaps in any other chain.

great fault. The Siwáliks were laid down to the south of the fault, while the hills to the north were still rising ; and probably in part while the fault was still forming. Subsequently, the Siwáliks themselves became involved and were raised to form the present Sub-Himalayan Chain ; and now they bear the same relation to the alluvial deposits of the Gangetic plain that the older rocks to the north of the fault bore to them at the time of their deposition. Probably, too, there is now a boundary fault between the Sub-Himalayas and the Gangetic plain itself.

In Kumaun and Gahrwál, where the structure is well known, there is not merely one of these faults, but a whole series of parallel faults which divide the hills into a number of belts ; and in each belt the newest beds are older than the newest beds in the belt to the south and newer than the newest in the belt to the north. In each case the fault practically marks the northern boundary of the newest rocks in the belt to the south of it, and the age of these newest beds probably gives us the period at which the fault was completed.

Thus the Himalayas gradually grew outwards from north to south, slowly encroaching on the area of deposition to the south ; and each of these great reversed faults marks a stage in the process. Oldham goes on to show how this mode of growth agrees with Fischer's theory of the formation of mountains ; but for this discussion I must refer to the account given in the *Manual*.

The question of the age of the Himalayas also is too large a one to be dealt with here. It depends mainly on what is meant by the term Himalayan Chain ; and it must suffice here to point out that the presence of Nummulitic rocks at a height of 18,500 feet above the sea [1] clearly proves that at least a very large part of the elevation has taken place since the commencement of the Tertiary era.

PHILIP LAKE.

[1] La Touche. "Re-discovery of Nummulites in Zanskar." *Rec. Geol. Surv. India*, xxi. (1888), p. 160.

CONTINUOUS-CURRENT DYNAMOS.

PART I.

ALTHOUGH it is the alternate-current dynamo which of late years has shown the most rapid growth both in theory and use, yet its closely allied partner, the continuous-current machine, has not been without steady progress, especially in the field of practice; it may therefore be of interest to shortly review its recent history, and thence endeavour to forecast the direction towards which future advances will tend.

The earliest dynamo which was invented belonged to the so-called "uni-polar" or "non-polar" class in which there is no reversal of the direction of the E.M.F. generated in the active wires or "inductors" of the armature. In spite, however, of its invention so far back as 1831, the uni-polar type finds to this day little or no practical use. Visions of a perfect dynamo, in which a truly continuous current is produced and collected without any reversal of its direction, and therefore without any likelihood of sparking at the brushes, have frequently been revived, and the idea has always presented great attractions. Yet often as the cry has arisen for such a dynamo without a commutator and free from all the troubles incidental thereto, it has never assumed any very practicable shape, the difficulty of generating any considerable voltage within reasonable limits of size having proved an insuperable obstacle to the ingenuity of inventors.[1] The electrical engineer is thus thrown back on the bi- or multi-polar dynamo with its two principal methods of winding the armature, which are identified with the names of Gramme and Siemens. The theory of the dynamo was in the main worked out from the scientific study and practical use of these machines between the years 1870 and 1885; but with the date 1886 we reach a most important stage in its history. In the *Philosophical Transactions of the Royal Society* for that year was published the classic paper of

[1] For a recent examination of several different types of uni-polar machine, see *Elektrotechnische Zeitschrift*, 11th Aug., 1893.

Drs. J. and E. Hopkinson on "Dynamo Electric Machinery,"[1] — a paper which may be said to have raised dynamo-designing from rule-of-thumb empiricism to the level of a scientific art. In it the authors for the first time laid down clearly and concisely the true method by which the voltage of a machine might be predetermined from its constructive details, and so accurate was their reasoning that subsequent theorising has largely consisted in the explanation and expansion of their conclusions. It is therefore instructive to consider modern progress by the light of this remarkable paper, and we may conveniently use its several headings to furnish us with starting-points for our outline of more recent results.

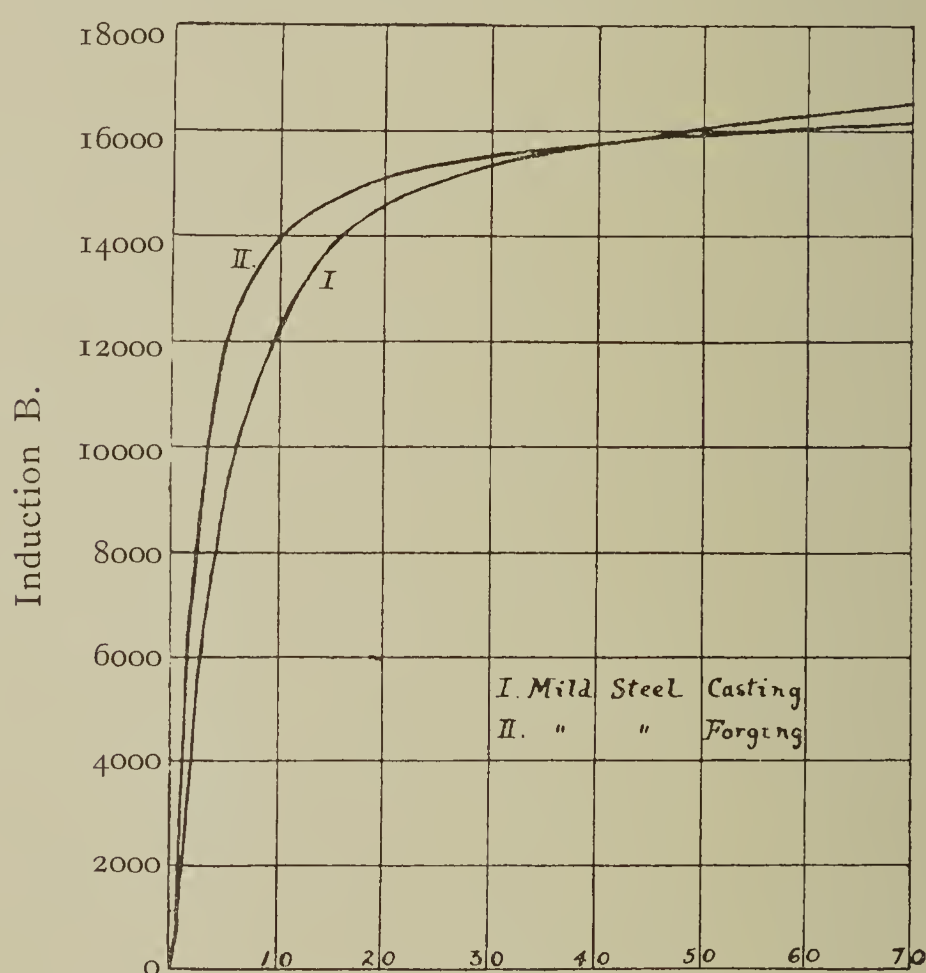
Its chief feature was an exhaustive analysis of the magnetic circuit of the ordinary bi-polar dynamo, and this led by the way to a consideration of the important question of the effect of the current in the armature on the magnetic field. Previous to it a number of formulæ had been invented for determining quantitatively the magnetism of a given iron circuit; one and all, however, had been tried and found wanting. The Drs. Hopkinson then took the designer back to the fountain source, Nature itself: determine by experiment the magnetic quality of the iron which is to be employed, and with the data thus obtained it will be possible to build up from the working drawings alone a complete predetermination of the voltage of the dynamo under any given degree of excitation. If the iron is the same as that experimented on, the answer must be correct.

The first essential then is a knowledge of our iron, and this now takes the form of "curves connecting B and H" for the particular class of iron used, or, in other words, curves from which for any given value of the magnetic difference of potential between opposite faces of a centimetre cube the number of C.G.S. lines which traverse it from one face to the other can be read off. Much against his will the iron manufacturer has been driven by the exigencies of competition to recognise

[1] Since reprinted in a collection of papers entitled *Original Papers on Dynamo Machinery and Allied Subjects* (Whittaker & Co.).

the necessity of having such curves constructed for his iron: fig. 1 shows two recent curves, the one for mild steel magnet forgings and the other for steel castings; the vertical ordinates represent magnetic induction and the horizontal abscissæ the corresponding magnetising force required to produce it. We are thus led to note the fact that improved metallurgical processes have brought about an apparent revolution in the nature of the material used for the magnets of dynamos. A few years ago, annealed wrought iron was the favourite material, and this was made of hand-picked scrap

Fig. 1.—Curves of exciting power and induction for mild steel.



Magnetising force H in C.G.S. units.

iron forged by hammering into large slabs. While hard steel was used for permanent magnets owing to its power of retaining its magnetism, the presence of steel in the forgings was regarded as well-nigh fatal to their magnetic value owing to its low permeability, and it was claimed that any admixture of this substance was best avoided by the process of hand-picking. Now, mild steel is freely used, and the question may be asked: How is it possible to reconcile present and past practice? The answer turns upon the vagueness of the term "steel"; in former days it

chiefly implied hard steel with a high percentage of carbon, a material which reaches its highest excellence in crucible cast steel. Gradually, however, the percentage of carbon has been so far reduced, and the softness so much increased that the same name of steel covers what is in effect as nearly pure iron as it is practically possible to produce. It is therefore mild or soft steel which is now so largely in use for dynamo work, and, so far as the purposes of the electrician are concerned, this is practically the same substance as was previously used in the form of wrought-iron forgings, the only difference being that whereas in the latter the iron was worked up into slabs by welding under the hammer, it is now produced in large quantities by melting in the furnace or converter and is then poured out into ingot or other moulds in which it solidifies. In the future we may expect such steel to play an ever-increasing part, especially for castings of a somewhat complicated shape which have also to serve as the carriers of magnetic lines.[1]

Given then the necessary information with regard to our iron, there follows an examination of the dimensions of the magnetic circuit of the dynamo which we are designing. The length and area of each portion is dealt with piecemeal, and the necessary exciting power required by it separately calculated; the final summing up of the various items so obtained enables us to draw a curve from which we can confidently predict how many useful lines will pass through the armature for any given value of the exciting power on the field-magnet.

The second important point of the paper of the Drs. Hopkinson was their treatment of the magnetic leakage in the case of two typical machines, the one of the Edison-Hopkinson type, and the other a Manchester dynamo with double horse-shoe field. In the first place, they experimentally determined the proportion which the lines straying beyond the limits of the armature bore to the number which actually traversed it, and were usefully cut by the wires on the surface: a factor (denoted by the symbol ν) was thus

[1] See a paper by Kapp, "On Methods of Testing the Magnetic Qualities of Iron," *Journal Inst. El. Eng.*, vol. xxiii., No. 110, 1894.

obtained, by which the number of useful lines has to be multiplied in order to determine the total number of lines passing through the field-magnet, and thence is calculated the density of the lines in the field-magnet. It was, however, pointed out that the value of the factor would vary according as the armature was more or less saturated, and shortly afterwards in the same year the magnetic leakage was dealt with in an even more complete manner by Mr. Kapp in a paper read before the Society of Telegraph Engineers [1] and also by Prof. Forbes in the discussion which followed. By the aid of their reasoning we are enabled to obtain a closely approximate figure for the number of leakage lines even when dealing with a new and untried type of dynamo. Numerous experimental determinations of the magnetic leakage in various makes of dynamos have since been published; [2] and much ingenuity has been expended in designing "iron-clad" dynamos, in which the field-magnet coils are enclosed in or shielded by iron in order to reduce the leakage to a minimum. It certainly is inconvenient to have our watches magnetised and our measuring instruments affected by dynamos at work near at hand; but experience has shown that it is easy to pay too high a figure in initial cost for the advantages of a dynamo with a small external field, except, of course, under special circumstances. One such circumstance is the use of large dynamos on board ship, where they are apt to disturb the compasses. This effect has manifested itself in a disagreeable manner in the case of the ships of H.M. Navy. Not only have the compasses been affected by dynamos, although situated at some distance away, but it is further stated that orders have been issued forbidding the carrying of bayonets by marines when on duty near working dynamos, since these iron arms become magnetised by induction, and then subsequently when brought near the compasses may deflect them. Quite recently the authorities of the British Admiralty have in-

[1] *Journal*, vol. xv., No. 64, 1887.

[2] *E.g.*, "Magnetic Leakage in Dynamos and Motors" (Ives), reprinted in the *Electrical Review*, 22nd and 29th Jan., 1892, and "Magnetic Data of Sprague Motor" (Parshall), *Electrical Engineer*, 13th June, 1890; also (Puffer), *Electrical Review*, 15th April, 1892

sisted on the dynamos constructed for their use being rendered practically innocuous in this respect by their being designed specially so as to have a negligible external field. In the case of small dynamos, which require proportionately small excitation, the difficulty may be met by the use of several comparatively thin iron screens enclosing the dynamo and separated from one another by intervening air-spaces. Such a device has been employed in a dynamo specially constructed for the Greenwich Observatory,[1] where delicate tests showed that its working did not affect the neighbouring instruments. When, however, we have to deal with large dynamos of the ordinary two-pole type, the remedy is by no means easily found, and it would seem that the only

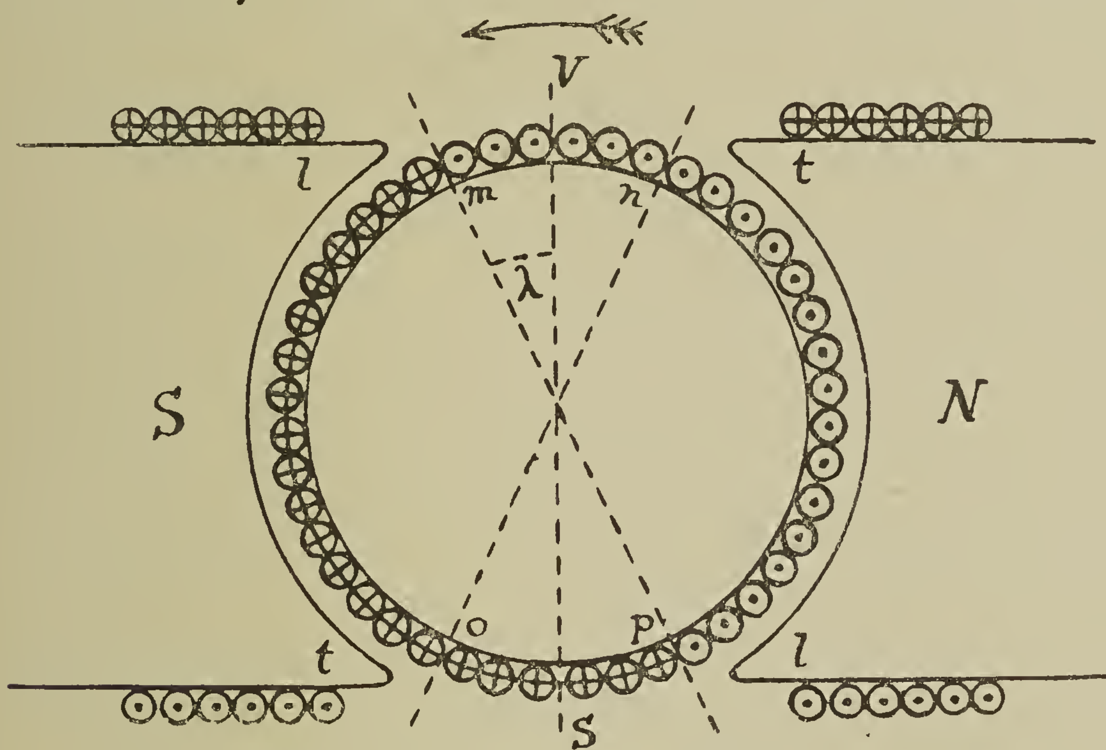


FIG. 2.—Back and cross Ampere-turns of Armature.

solution lies in a large reduction of the exciting power through the use of a very small air-space between the iron of the armature and the iron of the pole-pieces.

The next point on which again it is instructive to refer to the paper of Drs. J. and E. Hopkinson, is their examination into the reaction of the current in the armature upon the magnetic field of the dynamo. Considering for simplicity a bi-polar drum armature of the ordinary type, they showed that the actively inductive portion of the winding which lies on the surface of the core parallel to the pole-pieces has two magnetic effects which are of very different character. If λ (fig. 2) be the angle of lead of the brushes by which

[1] *Electrician*, 25th Aug., 1893.

they are displaced ahead of, or behind, the line of symmetry, VS, between the poles, the convolutions or loops included within the arcs *mn* and *op* have a direct magnetising effect, which will either increase or decrease the total number of lines passing through the armature according as the current-sheets embraced between the points *m*, *n* and *o*, *p* flow in the one or the other direction. The remaining convolutions included within the arcs *mo*, *np* were shown to produce "a material disturbance in the distribution of the induction over the bored face of the pole-piece": if the edge of a pole by which an inductor first enters into its field be called the "leading" pole-tip as opposed to the "trailing" tip by which it leaves the pole, then the lines are driven away from the leading corners *ll* and are crowded up towards the trailing corners *tt*. As, however, the weakening of the field at the leading pole-tip is counterbalanced by the strengthening of the field at the trailing pole-tips, "this disturbance has no material effect upon the performance of the machine". In the ordinary dynamo the brushes are advanced ahead of the line of symmetry in order to avoid sparking, *i.e.*, assuming the direction of rotation shown in fig. 2, to a position corresponding with the line *mp*. In this case, if the poles be as shown in the diagram, the current in the wires *m* to *n* is directed towards the observer as indicated by the dotted circles, and away from the observer in the corresponding wires to *op* as shown by the crossed circles. On comparing their directions with the direction of the current in the neighbouring wires of the magnet winding proper, it will be seen that they are exactly opposed to each other: in other words, the magnetising effect of the loops between *mn* and *op* is opposed to the magnetising force of the fixed coils on the magnet. They are, in fact, "back ampere-turns" neutralising an equivalent number of the forward ampere-turns of the stationary magnet-coils. On the other hand, the loops between *mo* and *np* are "cross ampere-turns" distorting the field by twisting it round in the direction of rotation. This division of the armature ampere-turns into two sets was brought into

greater prominence by Mr. Swinburne:[1] he showed that whatever the exact nature of the winding, the wires lying on the surface of the core may be regarded as united across the ends of the armature by imaginary connections into two sets of loops, lying in planes at right angles to each other. The subject was again advanced in two papers,[2] the one by the last-mentioned author and the other by Mr. Esson, until now the terms "back" and "cross" ampere-turns of the armature have become part of the orthodox terminology of the dynamo designer.

The final result of the various theoretical points to which brief allusion has been made above, may be summed up in the statement that the electrical engineer is now enabled to design a dynamo to produce a given voltage at a given speed with such accuracy that, when built and tested, its performance will not vary by more than two per cent. from that predetermined by calculation: such accuracy is obtained in common practice and within the limits of time that the busy routine of a large engineering factory permits of being given to the design.

The truth of the above-mentioned analysis of the effect of the armature current on the magnetic field was fully substantiated in another paper by Dr. Hopkinson appearing in the *Proceedings of the Royal Society*, 15th Feb., 1892. In this sequel to his former paper he gave an account of some experiments, in which the induction at different parts of the air-gap under the bored face of the pole-piece was measured. Taking two points, the one near the leading corner and the other near the trailing corner, he showed that within the limits of experimental error, the difference in the induction at those two points corresponded exactly to the difference in the magnetising forces due to the cross effect of the armature current which was flowing at the time of the experiment.[3] It was thus proved that, provided the iron

[1] *Journal Soc. Tel. Eng.*, vol. xv., No. 64, p. 542, Nov., 1886; also "Constant-speed Motors," *Electrical Engineer*, 20th Jan., 1888.

[2] *Journal Inst. El. Eng.*, vol. xix., part lxxxv.

[3] See also a paper "On the Relation of the Air-gap and the Shape of the Poles to the Performance of Dynamo-electric Machinery" (Ryan), *American Inst. El. Eng.*, 2nd Sept., 1892.

pole-pieces be far from saturation, the total number of lines passing through the armature is not affected by the cross magnetising force of its current. If, however, the pole-pieces have insufficient area and are therefore near saturation, the distortion of the lines may result in an actual diminution of their number owing to the trailing pole-corners being unable to carry the increased number which is crowded into them. This danger can, however, be obviated by proper design.

The real importance of the cross reaction lies in the fact that it weakens the fringe of lines which extends from the leading pole-tips. It is in this fringe that the section of the winding short-circuited by the brushes must be reversed, and the stronger the current which it is carrying, the stronger must be the field to reverse it; yet by the very action of the armature current the reversing field is weakened. It has thus been established that if the process of commutation is to be carried out successfully without sparking, the cross ampere-turns of the armature at full load[1] must not bear too great a proportion to the ampere-turns of the field-winding which are expended over the air-gap. Roughly it may be said that experience shows the advisability of not allowing the cross ampere-turns to exceed half the ampere-turns required by the air-gap, or, to put it in another way, the induction at the leading pole-tip must not be reduced to less than about 2500 or 3000 C.G.S. lines per sq. cm., *i.e.*, one-half of the average density over the whole of the polar air-space which usually ranges from about 5000 to 6000. Various methods have been suggested and tried for neutralising the effect of the cross ampere-turns of the armature,[2] but the only principle that can be said to be used in practice consists in keeping the length of the air-gap so large that the excitation which it requires is more than double the cross ampere-turns of the armature.

[1] *Journal Inst. El. Eng.*, vols. xix., part lxxxv., p. 95, and xx., part xciii., p. 268.

[2] See "On the Reaction of the Armature Current on the Field" (J. Fischer-Hinnen), *Elektrotechnische Zeitschrift*, 3rd Feb., 1893, and *Electrical World* (Ryan), 19th Nov., 1892.

This brings us to the great question of multi-polar *versus* bi-polar dynamos for large outputs. Given an armature core of a certain size, its output, so far as regards voltage and the current that may be obtained from it without overheating, is, roughly speaking, independent of the number of poles. As, however, we increase the number of poles, the cross ampere-turns acting on each pole are reduced in proportion: if therefore the number of poles be raised from two to four the length of air-gap required to prevent sparking may be reduced to half of that which would be required in the bi-polar machine.[1] Herein lies the advantage of the multi-polar type, but the question when the output is too large to be successfully obtained from a two-pole dynamo cannot be said to have been finally decided as yet. This point will be again alluded to later, and the fact is merely noted here that multi-polar forms have found much more favour on the Continent than in Britain.

The concluding remarks of the present article will be devoted to the efficiency of modern continuous-current machines. Viewed as a converter of energy from one form into another, the dynamo must certainly be pronounced the most wonderful machine which the engineer has at his command. Eight years ago, when Dr. Hopkinson determined by experiment the efficiency of an Edison-Hopkinson machine, he was able to show that when mechanical energy was imparted to the shaft of the armature at the rate of 57·6 H.P., electrical energy was delivered at the terminals in a useful form at the rate of 53·7 H.P., or an efficiency of over 93 per cent. In the waste power is included the loss due to the passage of the current through the resistance of the armature, the energy required for the excitation of the magnet, and the mechanical loss by friction of the bearings, and when we consider that all these losses must necessarily be present, however much we may succeed in minimising their amount, it will be seen how little room there is for improvement on the score of efficiency. Hence, in point of fact,

[1] "Notes on the Design of Multi-polar Machines" (Esson), *Journal Inst. El. Eng.*, vol. xx., part xciii.

although the efficiency above recorded has often been repeated and is now guaranteed under ordinary commercial conditions, it has seldom been surpassed, at least in machines of similar comparatively small size. The method devised by Dr. Hopkinson for testing the efficiency of dynamos is worthy of passing mention. Two similar dynamos are taken and are connected together either by a coupling or by belting; one of these, A, is arranged to work as a generator, and the current which it gives is supplied to the other, B, which consequently works as a motor. The mechanical energy delivered by the latter supplies the greater part of the driving power required by A; the horse-power of B as a motor must, however, necessarily be less than the horse-power required to drive A as a generator. This deficiency has, therefore, to be made up from some external source of power, and in the original experiments of Dr. Hopkinson it was supplied through a transmission dynamometer, which at the same time measured it. Thus the whole system is self-supporting, save for the necessary losses in the two machines under test, and from the power supplied to make up these losses, the efficiency of the two machines is easily deducible. The beauty of the method lies not only in the fact that, *e.g.*, two dynamos of fifty horse-power each may be tested under full load by means of a ten horse-power engine, but also in the directness with which the waste power is measured. A subsequent improvement of the same method consists in the use of a small subsidiary dynamo to supply the waste power electrically to the circuit: all the measurements can then be made electrically by voltmeter and ammeter, and a very high degree of accuracy is thus obtained.[1] When the dynamos of 240 kilowatts output at the Manchester central electric light station were tested by this method, efficiencies of 93·7 and 95·2 per cent. were found to have been obtained.[2] Of late years, owing to the demand for dynamos coupled directly to the crank-shafts of their driving engines for central-station work, much

[1] "The Determination of the Efficiency of Dynamos" (Kapp), *Electrical Engineer*, 22nd and 29th Jan., 1892.

[2] *Electrician*, 6th Oct., 1893.

attention has been paid to the over-all efficiency of the combined steam-engine and dynamo, or the ratio which the useful output at the terminals of the dynamo bears to the horse-power given out by the steam in the engine cylinders as shown by indicator diagrams. Two well-authenticated instances may be cited to show the present pitch of excellence. In a high-speed engine indicating 300 horse-power and directly coupled to a dynamo having an output of 1500 amperes at 120 volts, tests made at the Naval Exhibition of 1891 in London showed an over-all efficiency of 84 per cent., and on the assumption that the brake H.P. of the engine was 90 per cent. of the indicated H.P., an efficiency is found for the dynamo of 93·5. With a similar engine of 200 I.H.P. coupled to a dynamo for 125 volts and 1025 amperes at 350 revolutions per minute, an over-all efficiency of 85·6 has been obtained, the dynamo alone having the remarkable efficiency of 95·6 per cent. On the Continent, where large multi-polar machines running at considerably lower speeds are largely used, the over-all efficiency of the combination is generally about 82 per cent., this somewhat lower figure being accounted for partly by the slower speed and partly by the fact that the numerous poles demand a greater expenditure of exciting energy than would be the case if their lines were concentrated into a single magnetic circuit.

Finally, it is worth remarking that such high efficiencies as have been mentioned above are not obtained at the expense of the all-important quality of durability. It is often said that an extra 1 per cent. in the efficiency is dearly bought at the sacrifice of strength in some part of the machine's frame. But this reasoning, true though it is in many ways, loses much of its force when applied to the case of dynamos, and for a reason which is easily explicable. All the electrical and magnetic losses manifest themselves in the end as heat, and so tend to raise the working temperature of the dynamo: now one great guarantee that a machine will be durable is that its temperature remains low, so that its insulation is not likely to deteriorate from the effects of heat even after many years of hard work. Hence,

when the designer succeeds in reducing one of these losses, he is also rendering it easier to produce a dynamo which keeps cool in continuous work, and which is therefore more likely to be durable. A striking instance of this may be seen in the precautions which are now taken to eliminate eddy or parasitic currents, whether in the armature core itself or in the winding. In large machines the armature bars are now very commonly composed of several separate strands, lightly insulated from each other, twisted together and gently compressed into a rectangular section.[1] By this construction any single strand continually passes from one side to the other of the bar: hence, if the field in which the bar is moving varies in density within the limit of its breadth, each strand is partly in the stronger field and partly in the weaker. Thus no one strand produces a higher E.M.F. than any other, and the tendency for a local current to be set up within the bar when it enters or leaves the field of a pole is entirely eliminated.

C. C. HAWKINS.

[1] Crompton's patent, No. 12,880, 1886. For a valuable method of experimentally measuring the amount of the eddy currents in a wound armature, see *Electrician*, vol. xxvi., 1891, pp. 699, 700 and 740, and vol. xxvii., p. 162.

(To be continued.)

ON THE MORPHOLOGICAL VALUE OF THE ATTRACTION-SPHERE.

PART I.

PROBABLY no more rapid biological advance has ever been made than that which is at present gradually bringing into view the extraordinary structural complexity of a single cell. The ceaseless energy with which workers of all nations have attacked the cell problem will leave a standing monument to the value of anticipatory theories (however visionary) as a stimulus to research. Indeed it is through such speculative promptings that, as Whitman (1) lately put it, "all the search lights of the biological sciences have been turned upon the cell. It has been hunted up and down through every grade of organisation, it has been searched inside and out, experimented upon and studied in its manifold relations as a unit of form and function. It has been taken as the key to phylogeny and ontogeny, and on it theories of heredity have been built." Nor has the consequent increase of knowledge been confined to any one department of cytological research; but has affected the whole range of cellular existence, beginning in the protozoa and ending with the complex tissues and formed material into which cell-structure breaks down in the higher differentiation of the compound types.

Among other things it has been made abundantly evident that the rough though time-honoured division of a cell merely into nuclear and cytoplasmic constituents is no longer sufficient, and must be revised in accordance with the study of cellular development, while it is at the same time strangely interesting to see that under the influence of this new study the truth of old conceptions has not been lost, but reappears in the ever-increasing importance we attach to the view, that all cell-structures, however complex, can in their origin be resolved into nuclear and cytoplasmic derivatives, for such a conception is now the almost universal

key-note of cellular morphology. Much labour has been spent in attempting to determine a boundary line between such nuclear and cytoplasmic series proceeding on the assumption that structures outside the nucleus are cytoplasmic, whether they persistently exist or arise in that position, so long as they are not produced from dismembered portions of the nucleus, the inverse of this definition being applied to structures which are nuclear.

In its simplest form the cytoplasm (protoplasm) presents under a sufficiently high magnifying power the familiar, fine reticulum which has been interpreted in many ways. Perhaps the simplest, and certainly the most comprehensible, hypothesis respecting its undifferentiated structure is contained in Bütschli's (2) conception of a "Schaumplasm," *i.e.*, it consists of an immense number of fluid globules immersed in a non-miscible fluid, so that in optical or actual section it presents the appearance of a fluid reticulum. Under the action of stains, the meshes of this net-work colour less deeply than the strands, the stain being absorbed by fine granules (microsomes) along their course, while the fluid of which they are composed remains comparatively clear. The manner of coloration of some of the microsomes is similar to that of the interior nuclear mass, which also in its normal condition is built on the same general plan, *i.e.*, when stained, it consists in optical section of a reticulum, the colour being absorbed here by the chromatin granules, which are immersed in clear threads of fluid (linin). All cells show this gross structural division into nuclear and cytoplasmic groups, and although either may be more or less masked by the evolution of secondary structures during ontogeny, they form the ground-work of cellular organisation through every phase of life-history and metamorphosis. They are the elements out of which these superadded structures are evolved, and into which they again break down at the close of individual existence. Of late years, however, much less conspicuous, and less easily demonstrable cellular constituents, have been held by many, as probably of the same prime order. It is almost exactly twenty years since the able and vanished naturalist Hermann Fol (5) drew attention to

appearances of radiation round a point in the eggs of certain jelly-fish (geryonidæ) as being worthy of remark: "Setzen wir aber gerade in diesem Augenblicke etwas Essigsäure hinzu, so kommt der Rest, gleichsam, nur eine Andeutung des früheren Kernes wieder zum Vorschein. Auf beiden Seiten dieser Kernüberbleibsel zeigen sich zwei Protoplasmaanhäufungen, deren dicht angesammelten Körnchen zwei regelmässige sternförmige Figuren darstellen. Die Strahlen dieser Sterne werden durch die in geraden Linien aneinander gerichten Körchen gebildet. Mehrere solchen Linien reichen von einem Stern oder Anziehungscentrum in einem Bogem zum andern, inden sie die Reste des Keimbläschens umfassen. Das ganze Bild ist äusserst klar und deutlich und errinnert lebhaft an die Art und Weise wie ausgestreuter Eisenstaub sich um die beiden Pole eines Magneten anordnet. An den Ränden gehen die Strahlen allmählig einerseits in das dünnere Protoplasmantze des Endoplasmas, andererseits in das dichte Ectoplasm über."

For a long time the existence of these appearances attracted little more than desultory comment, and it was not until fourteen years had passed in the gradual improvement of our methods of research, that the classical publication of van Beneden (7) revolutionised our cytological conceptions by showing that similar radial appearances were of prime importance in the early ontogenetic development of Ascaris and were probably to be regarded as cellular constituents of the highest rank. Not content, however, with merely demonstrating the existence and functional importance of his "sphères-attractives," the Belgian naturalist conferred upon them the title of "Organs of the cell," each in itself composed of several parts. For if eggs be killed and stained by the admirable, if somewhat heroic, method he adopted, the spheres present a conspicuous three-zoned appearance, caused by the presence of a central body, surrounded by a light space, and enclosed on the outside by a broad band of granular protoplasm. Under the adopted treatment the central body stains bright green, a little darker than the rest of the sphere, but it is not in the least distinct in staining capacity from numer-

ous other granules existing elsewhere in the egg. It has, in fact, as far as its optical properties go, all the characters of these "microsomes," being distinguished from them during divisional metamorphosis solely by the position which it occupies at the centre of affairs.

The sphere in Ascaris was the first to be minutely studied and described, and it has consequently served as a sort of type for comparison with those we are becoming familiar with on all sides. It is essentially composed of a large mass of differentiated protoplasm outside the nucleus, containing within itself a clear space and central body.

The spheres are known in the literature which deals with them under a great variety of names, "sphère-attractive" being practically synonymous (among others) with "Sphère-directrice," "Aster," "Archoplasm," "Polar-body," "Polar-corpuscle," and their German equivalents, "Peryplastes," with their "Daughter-peryplastes," "Microcentrums," "Spermocentres," "Ovicentres," "Nebenkerns," "Astrocentres," "Vitelline nuclei" and "Dotterkerns". But although these titles have arisen in the description of obviously homologous structures found in a great variety of eggs and tissues, they by no means always refer to the whole, or even to the same part of the three-zoned structure originally described by van Beneden. Thus the centrosome is always the central staining granule, and is exactly equivalent to "Polar-body," "Polar-corpuscle," "Daughter-peryplaste," and "Microcentrum" (Heidenhain's (8) term for multiple centrosomes in the aggregate). But how far "Ovicentres," "Spermocentres," "Astrospheres" and "Astrocentres," "Vitelline nuclei" and "Dotterkerns" represent this and nothing more, it is difficult to say, while such terms as "Sphère-directrice," "Aster," "Archoplasm," "Peryplaste" and "Nebenkern" certainly stand for the whole sphere, which, as in Ascaris, generally contains within itself a smaller clear space and centrosome. The terminology of Boveri (9) has the advantage of simplicity, since by his use of "Archoplasm" for the outer, and "Centrosome" for the inner constituent of the sphere, no mistake is likely to be made. But as the archoplasm and the centrosome

can exist quite separate from one another, even during rest, it would be well to find some term to comprehend all these structurally co-related parts wherever they may be situated in the cell.

In the following pages I have employed the term "Attraction-sphere" in this sense, shortly, "Sphere," reserving "Archoplasm" for the outer granular mass to which it was originally applied and "Centrosome" for the inner granule.

Whether, with van Beneden, we are to regard the spheres as organs of the cell depends a good deal upon what is meant by the term organ. In its original conception the word has a physiological significance, and in this sense the purposive nature of any structure constitutes its claim to be regarded as an organ. Consequently both the nucleus, cytoplasm, and all other structures functional in the maintenance of the cell, fall into the category of organs, the spheres with their very definite functions among them. That they are not so regarded by many histologists is nevertheless a fact; but this tardiness in the acceptance of van Beneden's views has really nothing to do with indefinite terminology arising from certain considerations respecting the *origin* of the spheres of quite a different nature.

It has become a biological postulate, engrained by the invariable nature of the evidence on which it rests, that a cell in its origin is possessed of a nucleus and cytoplasm, if nothing else. And it is equally certain that Virchow's aphorism, "*omnis cellula e cellula*," could be extended down to the origin of the cytoplasm from pre-existing cytoplasm, and the nucleus from pre-existing nuclei of previous generations. But this simple duality of structure, with which cells often start in life, may, and very generally does, give place to a greater complexity of parts, through the development of accessory structures, of which some Nebenkerns form typical examples. Whatever be the function of these bodies (and in many cases it is not known), they are often plainly derived, by direct metamorphosis, from either the nuclear or cytoplasmic substance. They do not come in with the prime constituents at the outset, but are

evolved during the course of cellular ontogeny. On the other hand the nucleus and cytoplasm, as such, are never so evolved. Their relative proportions may increase or diminish without limit, but they neither arise *de novo*, nor are they ever entirely suppressed, themselves constituting the raw material out of which any future structural complexity is evolved. Until lately, the difference of structure apparent in the presence of the nucleus and cytoplasm was the only cellular organisation of which anything definite was known, and as both these constituents are typically *persistent*, it became customary to regard cell-organs as things always arising directly by fission from pre-existing similar structures. In other words, we have acquired the habit of regarding *persistence* as characteristic of cellular organs. No sooner, therefore, was the presence of spheres demonstrated than the question presented itself—Do these structures arise by division from pre-existing spheres, or can they, unlike the prime factors, be evolved during cellular ontogeny? And though this question of their origin is of the utmost importance, it appears to me that with respect to the nature of the spheres as cell-organs it has in reality nothing whatever to do.

It remains then to determine if possible whether the spheres (quite apart from their character as organs of the cell) have thus an *evolved* or *persistent* nature. How do they arise in the sexual elements? Are they persistent, but restricted to the male, dying out in the ova; persistent, but restricted to the female, dying out in the spermatozoa? Or do they arise from both, and fuse in the egg like the nuclei in the formation of a completely fertilised cell?

Finally, can these organs ever be formed anew, and so lose their claim to persistence and a place among the prime factors of the cell?

Van Beneden regarded the spheres as arising simultaneously, immediately after the entrance of the spermatozoa into the egg: "Les deux sphères apparient simultanement, si parfois on croit n'en voir qu'une, cela depend de la position relativedes deuxorgansrelativement à l'observation". In this case they do not appear to be present at the outset

either in the ova or in the spermatozoa, but to arise immediately after fertilisation, as the first organs evolved during individual ontogeny. The increasing amount of evidence which bears directly on this point has produced no other result so far than that of equally increasing the already wide divergence of opinion respecting the origin of the spheres in the first instance. Some observers, like Vejdovsky (10) (in the case of Rhinchelmes), believe that they enter with spermatozoa already pre-formed into the egg, having apparently arisen from pre-existing spheres in the spermatogenesis. Julin (11) regards the possession of a sphere as a distinctive character of the male cell of Stylopsis grassolaria, and goes so far as to make this a mark of distinction between ova and spermatozoa in general.

The researches of Balbiani (12) show the Dotterkern, "Vitelline nucleus" of Arachnid eggs, to be homologous with what he terms the "Nebenkern (Centrosome of Platner)," *i.e.*, of the attraction-sphere of these eggs, and to arise from the *nucleus!* as a little bud, which, although sometimes dividing, rapidly becomes "hypertrophid" and functionless, constituting a homological stepping-stone between those forms where the sphere appears to be present solely in the male, and those in which it is present only in the female.

Boveri in his last publication maintains that there are neither centrosomes nor asters related to the mitoses which form the polar bodies in the ovigenesis of Ascaris, and he uses this observation to support his theory that the spheres are derived entirely from the male.[1]

In the spermatogenesis of certain mammalia and elasmobranchs I (13, 14) have found bodies answering to the centrosomes to be incorporated with the mature spermatozoa, and Field (16) seems to have arrived at similar conclusions with respect to the spermatogenesis of echinoderms. Fick (17) figures a sphere related to the intruding spermatozoon

[1] While at the Naples laboratory last spring, my friend Dr. Morton Wheeler showed me preparations of the eggs of myzostoma with beautifully defined centrosomes at the apices of the polar-body spindles, so that Boveri's view is not of universal application.

in the fertilisation of the eggs of Axolotl, while indirectly Hermann (18) and others have arrived at similar conclusions by showing the "Mittelstück" to be a derivative of the Nebenkern in several spermatogenetic series, a structure which there is every reason to believe to be the archoplasmic portion of the attraction-sphere. There can indeed be very little doubt that sometimes at any rate spheres do pass into the ovum from the male as the descendants of pre-existing spheres in the antecedent generations of the spermatogenetic series. Others, however, like van Beneden, still hold that the spheres can be evolved in the egg, without any intervention on the part of the spermatozoon! and in the case of Ascaris they have not yet been shown to be derived from pre-existing spheres in any way.

And there is still a third, or intermediate class of observers, who conclude from the types they have examined that the spheres are produced in the first instance from both male and female cells alike, being presumably derived from pre-existing spheres in the spermato- and ovogenetic series, "tous les astrocentres du descendant, étant dérivés par divisions successives des astrocentres primitifs, se trouvent provenir, par parties égales, du père et de la mère," says Fol (6), who, in his famous and much-criticised "Marche du quadrille," figures an aster and centrosome related to each pro-nucleus in the fertilised eggs of sea-urchins, maintaining that each sphere divides before the formation of the first segmentation spindle, one-half of his "Spermocentre" uniting with one-half of his "Ovicentre" to form a new attraction-sphere at each end of the first spindle-figure.

More recent observations, like those of Konklin (20) on the zoological, and Guignard (21) in his notable paper relating to the fertilisation of Lilium Martagon, on the botanical side, have arrived at substantially the same results; although it is possible that the figures supposed to be indicative of fusion in these cases admit of other interpretations.

A vast importance attaches to the supposed fusion of the spheres, since it not only presupposes persistence in both sexes for these structures but suggests that they may have

as important functions in relation to hereditary phenomena as the nuclei themselves. In order, however, to really claim this for the spheres it would be necessary to find at least as strong evidence for their universal bi-sexual origin and fusion during fertilisation as we have in the case of the nuclear elements. Yet it is sufficiently apparent that nothing of the kind is forthcoming, that it is impossible to formulate any rule having the faintest pretence to be general respecting the uni-sexual or bi-sexual origin of the spheres; and we are equally baffled by the want of unanimity in recorded observations when we try to determine whether the spheres in their origin are always derivatives of pre-existing spheres. Of the two alternative answers to the first question, however, one must be true. Either the spheres have an altogether mixed origin, and can arise indifferently from the male or female, or be evolved in the egg at the time of fertilisation, and are *ipso facto* excluded from any hereditary value whatever, or those observations which tend to show that they originate in this way are incomplete, and with more extended knowledge their bi-sexual nature and fusion will be established throughout. Nor should it be forgotten that the evidence which militates against this bi-sexual origin is of one kind only. It consists of the non-forthcoming of one or other of the figures of that "Marche du quadrille," as Fol originally described it; and it appears consequently at first sight to be negative.

With respect to the persistent or evolved nature of the spheres, it will, moreover, be obvious that these structures may be persistent, although arising from one parent only; and the only direct evidence against their persistence in this sense is that which supports the view that they are evolved *de novo* in the egg, and is at present very slender. Undoubtedly attraction-spheres are brought in by the spermatozoa in some species of animals, whatever may be the case in plants, while bodies answering to them are incorporated with the male element during spermatogenesis or its equivalent in others. Evidence which is really positive has greater weight than any amount of that which is merely negative, and on the strength of this, probability seems to set towards

the persistent as well as bi-sexual nature of the spheres. But on closer inspection of the facts it does not seem to me that the evidence against persistence is really altogether of a negative character. If the spheres have an altogether mixed origin arising from either sex or being evolved *de novo* in the egg, the appearances would be exactly those which have been hitherto recorded. Consequently these can be and have been used as positive evidence of a mixed origin. Nor can there be any doubt that the attractive speculations which sprang from the suggestive observation made by Fol, and its startling confirmation in Guignard's paper which followed close upon its heels, have exercised an inevitable tendency in swaying the minds of observers towards a confirmation of these views which are legitimate only if the "Marche du quadrille" is the tune to which the centrosomes invariably "dance" through the initial phases of ontogeny. There are, however, other sources of evidence bearing on these vexed questions, of which I shall treat successively; one involving our knowledge of the life-history of the spheres in successive generations of tissue-cells, and another the as yet too scanty observations relating to the same phenomena in several protozoan animals and plants.

J. E. S. Moore.

(To be continued.)

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INHIBITION.

OUR positive knowledge of inhibitory phenomena, and still more our speculative forecasts, more or less closely dependent upon such added knowledge, have of late years greatly extended themselves. The term itself, first used to denote the now familiar phenomenon of cardiac inhibition, has expanded far beyond its original clear boundaries, to include, not merely all cases where one positive action is arrested by another positive action, but also cases in which we had been satisfied to see mere cessation of action or paralysis by defect. The familiar instance of cardiac inhibitory nerves has led us to look for and find inhibitory nerve action wherever a tissue becomes less active, and in the case of every cessation of action to see not merely a fall of motor action but a rise of anti-motor action. The need for some rational theory of inhibitory action in the well-known case of the cardiac vagus, in the hardly less well-known phenomena of vaso-dilatation, in the whole range of nerve-centre phenomena, has led to doctrinal conceptions apparently most disconnected from an experimental base of departure, yet most provocative of experiments made for the definite purpose of providing such experimental base. The clear case of motor and anti-motor cardiac and vascular nerves has invited us to admit as a probable generalisation that all tissues are controlled by excitatory and by inhibitory nerves. And explicitly or implicitly this generalisation has been operative in the most

apparently disconnected provinces of physiological research and speculation. It has expressed itself most clearly in connection with optical phenomena, with cardiac and vascular phenomena, with the pupil, with glands, and finally with voluntary muscle.

It is hardly possible within the limits of a short article to do more than indicate by a few allusions and headings the present drift of experiments that may fitly be grouped together as illustrating inhibitory phenomena, and supporting more or less cogently the group of speculations that enter into the modern doctrine of inhibition.

With regard to the inhibitory action of the cardiac vagus and the similar less clear but more general case of vaso-dilatator nerves, we are still to-day, as was the case thirty years ago, in presence of two theories, neither of which can be admitted to have finally established itself in undisputed possession. On the older theory an inhibitory nerve (vagus, chorda tympani) takes effect by acting on peripheral nerve-cells, organs of intermediation between nerve-fibres and muscular fibres; on the newer theory they depress activity by promoting the constructive or anabolic phase of nutrition. There is but little positive evidence in support of this second theory; the reversed electrical variation in cardiac muscle due to excitation of the vagus nerve may indeed be due to a reversed trophic movement within the muscle, yet can hardly be admitted as cogent evidence in that respect; we know that the nerve has an anti-tonic, anti-motor effect, and that the variation with such anti-motor effect should be the reverse of that witnessed with a motor effect, need not excite our surprise. Nor have attempts to detect a reversed thermic effect with vagus action led to any more significant results. On the other hand—and this is a consideration that strangely enough seems to have been wholly overlooked—the histological generalisation, confirmed especially by Gaskell's labours, is precisely most in harmony with the older view, and in the scale therefore opposite to that of the newer view; if the old distinction, made especially with reference to vascular nerves, between the cerebral or medullated nerve (vagus and chorda tympani)

and the sympathetic or non-medullated nerve (vaso-constrictors and accelerantes cordis) be admitted, if, further, we admit as proved that the former, which are anti-motor in action, are medullated down to their distribution in a distal series of ganglion cells, while the latter, which are motor in action, are non-medullated in nerve-trunks, having lost their sheath in a proximal series of ganglion cells, it seems a shorter step to admit as probable that an inhibitory nerve-fibre is anti-motor to cardiac and to vascular muscle by virtue of an interference action consummating itself in a distal ganglion cell, than to refer cardiac rest and vascular relaxation to a direct anabolic effect of nerve upon muscle.

In France a somewhat similar question, though differently formulated, has long since been raised.

Bernard never abandoned his first idea concerning the action of the sympathetic, to the effect that it had an inhibitory action upon the tissues, arresting their function, extinguishing, so to speak, or, rather, damping, their combustion, that it was a frigorific nerve. Until the end of his life he maintained that the rise and fall of temperature in the rabbit's ear after section and excitation of the cervical sympathetic were *not* vascular effects, but the consequence of the suppression and exaggeration respectively of a "frigorific" action of the sympathetic. He thus anticipated, and upon experimentally very impure data, the view which is raising its head at the present day, to the effect that the tissues of the body, especially the muscular tissues, are controlled, as well as excited, by nerves, put to rest by anabolic or inhibitory nerves, put into motion by katabolic or motor nerves. But in this view the sympathetic, regarded as the type of a katabolic nerve in England, is presented to us an anabolic nerve. Morat has re-examined under more thoroughly insulative conditions one of the cases relied upon by Bernard in support of this thesis, *viz.*, the case of the submaxillary gland with its two nerves. Bernard had discovered on this gland the well-known effects, *viz.*, cooling by excitation of the sympathetic, warming by excitation of the chorda, but, refusing to admit with Brown-Séquard and Waller that these were effects of vascular constriction and

dilatation, he inferred that the sympathetic and chorda have direct frigorific and thermogenic actions respectively.

Morat finds that, after elimination of the vascular modifications, excitation of *both* the glandular nerves produces a rise of temperature, a result that is consonant with the fact that both are secreto-motor nerves. Proceeding to look for a frigorific effect of an inhibitory nerve, he examines the effect of vagus excitation on the temperature of the cardiac muscle, and finds, as was to be expected, that depression or arrest of the beats is accompanied by a fall of temperature.

In Germany the search for inhibitory effects has been pursued mainly in consequence of the impulsion of Hering's [1] well-known doctrine of assimilatory and disassimilatory processes, first in the retino-cerebral apparatus, then in all tissues; the search has been directed more especially to the voluntary muscles and nerves of the lower animals and of man himself. Biedermann's experiments on the claw of the crayfish and on veratrinised frog's muscle—Kaiser's glycerine experiment on the frog's sciatic—Wedensky's observations also on the frog's sciatic—the investigations of Fick, of Mosso and of Waller on human nerve and muscle, contain data bearing more or less closely upon the question. It is also touched upon by the experiments *re* dilatation of the pupil, by Heese and by Langley and Anderson, and by an observation of Sherrington's with regard to movement of the eyeballs. We might also have mentioned the observations of Orchansky, but that the question of central inhibitory phenomena is too extensive to be properly included in this brief article, which is intended to cover only the question of peripheral inhibitory phenomena.

Biedermann's observations are now generally known; they need, therefore, merely be attended to as among the first definite data giving body to the floating notion of motor states, antagonised and overcome by anti-motor processes consummated in the muscle itself. The main point

[1] Hering, *Zur Theorie der Vorgänge in der lebendigen Substanz.* Prag., 1888.

of these experiments (which do not broach the question of the existence of specific inhibitory nerves) seems to be that as excitation may replace the quiescent by the active state so also may excitation replace the active by the quiescent state. The same electrical excitation causing contraction of relaxed muscle, brings about the relaxation of contracted muscle (veratrinised). In this case we have to do with direct excitation of muscle. In the recent observation of Kaiser (and also in the older, if less precise, observations of Schiff) we have to do with muscular relaxation by indirect excitation—a muscle put into tonic contraction by chemical excitation of its nerve (by glycerine) is made to relax by tetanising induction shocks also applied to nerve.

These are perhaps the least ambiguous data in the direction of the much sought for proof of the existence of muscular inhibitory nerves; they cannot be regarded as satisfactory, but in comparison with other data to which appeal has been made they are clearness itself. Hering's presentation of anodic depression of nerve excitability as a case of inhibition, Mosso's observations on human muscle, Wedensky's experiments on frog's muscle, have very slight and distant connection with the question, and are at best suggestive of many various possibilities.

To draw the conclusion that there are inhibitory nerves to muscle from the fact that the electrical excitation of nerve may interfere with and diminish voluntary and artificial muscular contractions is a step requiring considerable credulity. The datum is, in fact, even more complicated by possibilities than an analogous datum under more simplified conditions that was brought forward by Schiff[1] forty years ago, and rejected by him as fallacious evidence of musculo-inhibitory nerves. The experiment is a well-nigh forgotten one, and may therefore fitly be redescribed as "new"—the nerve of an ordinary nerve-muscle preparation is tetanised by induction shocks until the muscle ceases to respond; at this time single induction shocks applied to another part of the nerve at intervals sufficiently long to allow of recovery

[1] Schiff, *Physiologie des Menschen*, p. 188. Lahr, 1858.

coming into effect, will cause single muscular twitches, and if during a rhythmic series of such twitches the original tetanising current is remade, the twitches are temporarily inhibited just as if the twitching muscle were a contracting ventricle and the sciatic nerve its inhibitory nerve. The experimental fact is clear and elegant, but does not bear the conclusion that would no doubt be placed upon it by special seekers for musculo-inhibitory nerves. Moreover, it has not been cleared of a possible fallacy that was found to be subversive of the somewhat similar experiments of Valentin and of Grunhagen.

If the idea of inhibition and of inhibitory nerves is not to become entirely dissipated, if we are to admit under that rubric the mere passive cessation of action as well as the active arrest of action, then all nerves are inhibitory, all paralysis is inhibition. And to any one convinced of the existence of efferent inhibitory nerves to skeletal as well as to vascular muscle it is no drawback that we do not know whether the cardiac vagus acts directly on the muscle or through the intermediation of ganglion cells, that we do know that all inhibitory phenomena effected through afferent fibres have their seat of consummation in nerve-centres, that the experimental demonstration of musculo-inhibitory nerves is most inconclusive of their existence. And it is possible to go further in the construction of suppositions, to suppose that not only muscle but all living tissues are under the control of direct inhibitory as well as of motor nerves, that inhibitory nerves are inhibitory because anabolic, motor nerves motor because katabolic. But the superstructure in its present state needs to be buttressed and underpinned by more cogent experimental facts than any that have recently been employed for the purpose. The case for the inhibitory nerves of skeletal muscle is unproven.

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THE NEW THEORY OF SOLUTIONS.

(*Continued from vol. i., p.* 419.)

FREEZING-POINT.

SINCE solid substance and liquid substance are in equilibrium at the freezing-point, in order that this state of equilibrium may remain undisturbed, the vapour-pressure of the solid must be equal to that of the liquid.

Now, when a solution freezes, experiment indicates that, in general, the solvent alone solidifies. The fact that drinkable water is produced by melting sea-ice supports this conclusion, and evidence of the same nature is afforded by the results of Fritzsche, who on freezing aqueous solutions of coloured substances obtained colourless ice, and by the striking observation made by Rudorff, who found that although solid magnesium platinocyanide had an intensely deep-red colour, yet the ice which separated from its aqueous solutions was colourless.

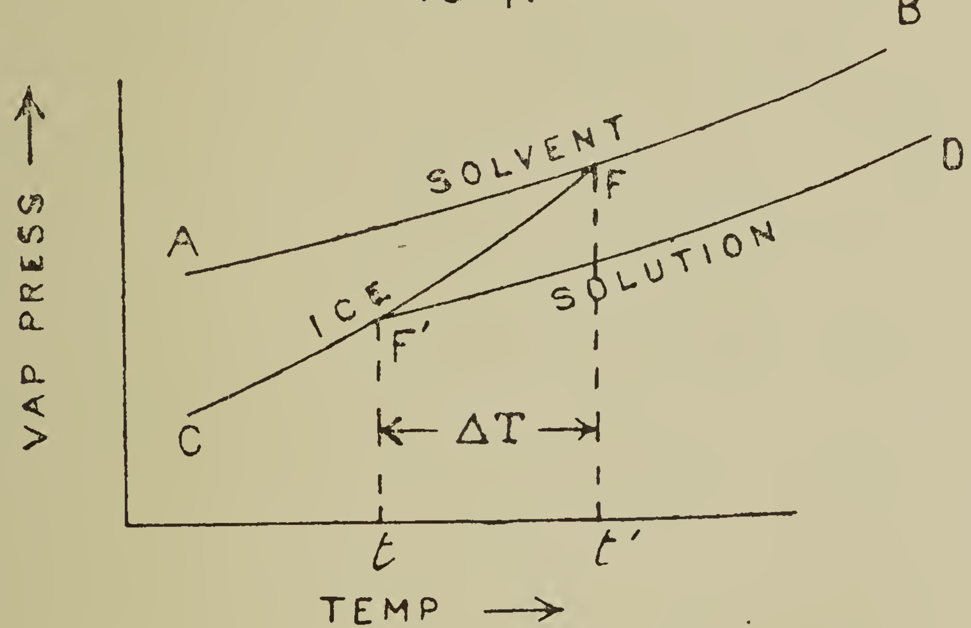
It may therefore be assumed that at the freezing-point of a solution the solid solvent and the solution are in equilibrium, and the freezing-point may be defined as the temperature at which the solution has the same vapour-pressure as the solid solvent.

It can be shown thermodynamically (and experiment justifies the conclusion) that at any temperature below the freezing-point the vapour-pressure of the solid solvent must be lower than that of the supercooled liquid solvent. In fig. 4, CF and AB may thus be taken to represent the vapour-pressure curves of the solid and liquid solvents respectively, and the temperature t', corresponding with the point F at which the curves cut, will be the freezing-point of the solvent. It has been already seen that the vapour-pressure of a solution is lower than that of the pure solvent at the same temperature; DF′ may therefore be taken as

the vapour-pressure curve of the solution, and F′ will correspond to a temperature t which is the freezing-point of the solution, for at F′ the vapour-pressures of solution and solid solvent are the same. It is evident from the diagram that t is less than t'; consequently it follows from the fact that the vapour-pressure of a solvent is diminished by the presence of a dissolved substance that the freezing-point is also diminished. The lowering of the freezing-point, $t'-t$ or ΔT, has been found to obey the following laws.

Influence of concentration.—The freezing-points of different solutions have only been investigated within comparatively recent times, although more than one hundred years ago James Blagden communicated to the Royal

FIG 4.



Society a series of observations on the freezing-points of aqueous solutions of simple and mixed salts, and announced the law, to which, however, there were exceptions, that the lowering of the freezing-point of the solvent was proportional to the amount of dissolved substance present in the solution. In 1882 Raoult began to employ solvents other than water. For solutions containing not more than 1 gram-molecule of substance in 1000 gr. of solvent, he found *Blagden's law* to be approximately true. Subsequent investigation by Raoult himself, and by other observers, in particular the extensive measurements by Beckmann and Eykman, have proved that, in general, even in dilute solutions, Blagden's law is not strictly obeyed. If, as before,

g be the number of grams of substance present in 100 gr. of solvent, the value of $\Delta T/g$ either slowly increases or slowly diminishes as g increases—the nature of the alteration being of the same order as that discussed in connection with the rise in the boiling-point.

Influence of the chemical nature of the dissolved substance.—The most important result announced by Raoult in his first paper was that, for solutions in the same solvent, the *molecular lowering* of the freezing-point is the same. If M′ be the molecular weight of the dissolved substance, $M'\Delta T/g$ is a constant, say F, which depends only on the nature of the solvent. This significant conclusion, published it has to be remembered in 1882, must be regarded as the first broad generalisation arrived at in connection with the stoichiometry of solutions. It gave the clue to the mode of treatment which has been applied with signal success in the case of other properties, as it showed that the solutions which could be most profitably investigated were those containing molecular quantities of dissolved material.

Since $\Delta T/g$ varies slightly with the value of g, the same must, of course, be true for the molecular lowering. Raoult has not particularly concerned himself with tracing this variation. It is illustrated in the following tables, wherein the observations for water are due to Arrhenius, and for acetic acid to Beckmann.

SOLUTIONS IN WATER.

Alcohol M′=46.			Ether M′=74.		
g.	ΔT.	M′ΔT/g.	g.	ΔT.	M′ΔT/g.
·575	·229	18·3	·87	·22	18·7
1·44	·591	18·9	1·74	·42	17·9
2·85	1·183	19·1	2·87	·73	18·8
5·70	2·456	19·8	5·74	1·51	19·5

SOLUTIONS IN ACETIC ACID.

Acetone $M' = 58$.			Ethyl Benzoate $M' = 150$.		
g.	ΔT.	$M'\Delta T/g$.	g.	ΔT.	$M'\Delta T/g$.
·502	·340	39·2	·878	·230	39·2
4·402	2·835	37·3	5·615	1·360	36·3
9·202	5·785	36·4	14·08	3·125	33·2
15·91	9·740	35·5	25·53	5·180	30·4

For the solutions given, the value of the molecular rise steadily alters as g increases. In the most dilute solutions, however, the values for different substances dissolved in the same solvent are practically the same.

Influence of the chemical nature of the solvent.—In 1882, Raoult concluded that F, the molecular lowering, is proportional to M, the molecular weight of the solvent. The data given by him in 1884 to support this statement are contained in the following table, where F is, for each solvent, the mean value obtained by the use of different dissolved substances.

Solvent.	M.	F.	F/M.
Water	18	47	4 × ·65
Formic Acid . . .	46	29	·63
Acetic Acid . . .	60	39	·65
Benzene	78	50	·64
Nitrobenzene . . .	123	73	·59
Ethylene dibromide . .	188	119	·63

The values of F/M do not differ widely from 0·63, and Raoult therefore stated the rule that "when one molecule of a substance is dissolved in one hundred molecules of any solvent, the freezing-point of the latter is lowered by 0·63°". In order to make water harmonise with this rule, the above table shows that its molecular weight has to be taken as 4 × 18, the molecular formula of liquid water being supposed to be $4H_2O$. The value of F for water had been derived

from the study of salt-solutions. The evidence given by them was, however, anything but satisfactory; indeed, it had previously led Raoult to assume $3H_2O$ as the formula of liquid water. Eventually he concluded that the constitution of saline solutions was exceptional, and he adopted 20·3, the value of F given by solutions of indifferent organic substances, as the true value of the molecular lowering. The molecular formula of water had now to be assumed to be $2H_2O$ in order to get a value of F/M agreeing with the rule. The assumption that the molecular complexity of a solvent exercises a definite effect upon the freezing-point of its solutions, is open to the objection that other correlated properties give no indication of being influenced in a similar way. Moreover, any independent testimony which may be taken to prove that the molecule of liquid water is complex, also points to the conclusion that the same is true for formic acid and acetic acid. Hence the molecular lowerings of these solvents as well as that of water should be exceptional if the freezing-point is affected by molecular complexity. The preceding table shows, however, that the acids exhibit no peculiarity. For these and other reasons there is little question that Raoult's explanation of the behaviour of water is incorrect; water is therefore a marked exception to his rule, and in the light of more recent work it is almost certain that the rule is erroneous. What appears to be the correct value of the molecular lowering was deduced thermodynamically by van't Hoff in 1886. He showed that F is not proportional to the molecular weight of the solvent, but is given by the expression $F = \cdot 02T^2/W$, where T is the freezing-point of the solvent on the absolute scale, and W is its heat of fusion in gram-calories.

Although the observations of Fabinyi (1889) on solutions in naphthalene and thymol support Raoult's empirical rule, the work of Hentschel (1888), and in particular that of Eykman (1888-89), who has carried out a careful investigation of the question, have decided in favour of van't Hoff's theoretical value of the molecular lowering. The data which substantiate this conclusion are collected in the fol-

lowing table. In the second column are given the values, at extreme dilution, of the molecular lowering obtained from observations on the freezing-point of dilute solutions, made, for the most part, by Raoult, Beckmann, and Eykman. In the third column are given the values of the molecular lowering calculated from measurements of the freezing-point and heat of fusion of the solvents, made chiefly by Eykman, Berthelot, Pettersson, and Batelli.

Solvent.	$M'\Delta T/g$.	$\cdot 02T^2/W$.
Water	18·5	18·9
Formic Acid	27·7	28·4
Acetic Acid	39	38·8
Nitrogen Peroxide	41	37-43
Lauric Acid	44	45·2
Para-toluidine	52	49
Urethane	50	50
Benzene	49	51
Naphthalene	71	69·4
Nitrobenzene	70·7	69·5
Phenol	74	76
Azobenzene	82	83
Diphenyl	82	84
Thymol	83	85
Diphenylamine	88	98·6
Naphthylamine	78	102·5
Ethylene Dibromide	118	119

Unless in the case of diphenylamine and naphthylamine the agreement between the two sets of values leaves little to be desired. The discrepancy exhibited by these two compounds is very probably to be attributed to impurity in the samples used by Batelli for determining the heats of fusion. This assumption is based upon the low melting-points of his samples. Pure naphthylamine melts at 50°, and diphenylamine at 54°, whereas Batelli's preparations melted at 43·4° and 51° respectively.

There is very little doubt, therefore, that the law of the freezing-point of dilute solutions is given by

$$M'\Delta T/g = \cdot 02T^2/W. \quad (1).$$

Freezing-point and the gaseous laws.—Relationships between freezing-point and osmotic pressure may readily be established by means of the connections which exist between

each of these properties and vapour-pressure. Since at the freezing-point a solution has the same vapour-pressure as the solid solvent, solutions in the same solvent having the same freezing-point must have the same vapour-pressure. But solutions which at a given temperature have the same vapour-pressure have been shown to have the same osmotic pressure, consequently solutions having the same freezing-point must have the same osmotic pressure. From the fact that Raoult's determinations have shown that solutions having the same freezing-point have the same molecular concentration, we have another mode of proving that Avogadro's hypothesis is applicable to dilute solutions, or that isotonic solutions contain per unit volume the same number of dissolved molecules.

In order to ascertain how freezing-point observations accord with the general equation PV = ·0819 T, a quantitative connection has to be established between the lowering of the vapour-pressure and the lowering of the freezing-point. Such a connection was deduced as early as 1870, by Guldberg, who started with the well-known equations expressing the heats of vaporisation of the solid and the liquid solvents in terms of the pressure, temperature, and volume of the vapour to which each gives rise. Assuming the vapour to obey gaseous laws, Guldberg's relationship may be formulated as

$$\log_e p/p' = MW/2 \times (1/T' - 1/T) \quad (2).$$

Here p is the vapour-pressure of the liquid solvent, and p' that of the solid solvent, and therefore of the solution, at T', the absolute freezing-point of the solution. T is the absolute freezing-point of the pure solvent. W is the difference between the heats of vaporisation of 1 gr. of solid solvent and 1 gr. of liquid solvent, and is thus the heat of fusion of 1 gr. of solid solvent. If we now substitute for $\log_e p/p'$, its value at T' as given by the vapour-pressure equation (6) (vol. i., p. 414), we obtain on writing ΔT for $T-T'$

$$P = \cdot 0819\, \rho W \Delta T/2T^{*} \quad (3).$$

* In this equation W/2T is assumed to be the same at the freezing-points of the solvent and the solution. Arrhenius has shown that if the

The experimental work on the freezing-point has shown that $M'\Delta T/g = \cdot 02T^2/W$, consequently

$$P = \cdot 0819\ \rho g T/100M' \quad (4).$$

Since g grams of dissolved substance are present in 100 gr. of solvent, the volume, V, of solvent containing M′ grams of dissolved substance will be $100M'/\rho g$ litres. In a dilute solution, since the weight of the dissolved substance is small and the density of the solution differs little from that of the solvent, this may be taken as the volume of the solution. Hence equation (4) reduces to

$$PV = \cdot 0819T.$$

Equation (3) makes it possible to calculate the osmotic pressure from observations on the freezing-point; it indicates that osmotic pressure is independent of the chemical nature of the solvent; and just as in the case of vapour-pressure and boiling-point, the entire discussion demonstrates the validity of the gas-equation as applied to dilute indifferent solutions.

CONSEQUENCES OF THE VALIDITY OF THE GAS-EQUATION FOR DILUTE SOLUTIONS.

We have now considered the most straightforward evidence which can be adduced to prove that the equation $PV = \cdot 0819T$ is applicable to dilute solutions. As will have been seen, the validity of the application rests solely upon the results of experiment or upon deductions drawn from such results by the aid of well-established thermodynamical principles. It is independent even of any assumption as to whether osmotic pressure is to be ascribed solely to the dissolved substance. Since the data indicate, however, that osmotic pressure is proportional to the concentration of the solution and is not affected by the chemical nature of the solvent, it seems but reasonable to attribute this pressure to the dissolved substance alone, and hence, for the reason already given, it is regarded as the kinetic pressure of the dissolved molecules.

temperature-variation of W be allowed for, the equation still gives good results for the ismotic pressure at T, in the case of aqueous solutions where the lowering of the freezing-point is as high as 10°.

Moreover, since, according to experiment, osmotic pressure is equal to the corresponding gaseous pressure, it is again a plausible assumption that Avogadro's hypothesis may be literally extended to dilute solutions, and, therefore, that the molecular condition of gaseous and dissolved substances is the same. This is in reality the fundamental hypothesis of the new theory: in all dilute solutions to which the gas-equation applies, the dissolved substance is assumed to be present in the condition of simple molecules, and the behaviour of solutions to which the equation is inapplicable finds an explanation in the supposition that in them the molecular state of the dissolved material is exceptional. We are thus led to the conclusion that when the molecules of a substance are widely separated from one another, as in the case of a gas or a dilute solution, the pressure which promotes their diffusion depends solely upon their number and not upon their nature; it is, further, independent not only of the nature, but even of the presence of the solvent.

Osmotic pressure and diffusion.—Although this is so, the actual process of diffusion is very different in the case of a gas and a dissolved substance, on account of the enormous frictional forces, due to the presence of the solvent, which oppose the movement of dissolved molecules. On the supposition that osmotic pressure is the cause of diffusion (Nernst, 1888), this frictional force can be calculated. The osmotic pressure-laws give the value of the force promoting diffusion, actual observations give the velocity of diffusion; all the data are thus to hand for evaluating the resistance opposing the movement. In this way it has been found that in order to drive one gram-molecule of sugar through water at 18° with a velocity of 1 cm. per second, a force of no less than $4 \cdot 7 \times 10^9$ kilograms-weight must be employed. That the force should be so large finds an explanation in the extreme smallness of the moving particles—the molecules. Hence, although gaseous and osmotic pressure are equal in magnitude, the equalisation of the pressure of a quantity of gas is almost instantaneous, whereas the equalisation of the concentration and

consequently the osmotic pressure of a solution is a matter of extreme slowness.

Molecular weights and the properties of solutions.—Much of the testimony which substantiates the applicability of the gas-equation has been accumulated in the endeavour to utilise the properties of dilute solutions as methods of estimating the molecular weights of soluble substances which cannot be volatilised. It has again to be noted here that these methods are purely empirical. Direct and indirect experiments show that, for substances of known molecular weight, osmotic pressure is equal to the corresponding gaseous pressure. If this rule be extended, and we assert that the osmotic pressure of a soluble non-volatile substance may still be regarded as the pressure which it would exert if it could be vaporised, even in such cases we may estimate vapour density and hence molecular weight. But apart from any correlation of the other properties of solutions with osmotic pressure, it would be rational to employ observations on solutions to fix doubtful or unknown molecular weights. Oft-repeated experiment has proved that in the case of substances of known molecular weight, solutions of the same molecular concentration have the same vapour-pressure, boiling-point, and freezing-point; it is therefore only necessary to extend these results to solutions in general, in order to obtain the molecular weight of any non-volatile substance which is soluble in a suitable solvent. Although the use of these methods may thus be justified apart from theoretical considerations, it was not until van't Hoff's theory had been published that their value became widely recognised. Raoult's first series of observations on the freezing-points of indifferent solutions appeared in 1882. In 1884 he generalised his results and laid especial stress on the ease with which the observations lent themselves to the determination of molecular weights. The first attempt to turn the method to practical account was not made, however, till 1886, when Paterno and Nasini estimated the molecular weights of paraldehyde and dicyanamide from the cyroscopic behaviour of their solutions. In 1888 Victor Meyer drew attention to the great

practical importance of the results of Raoult's work, which he regarded as the most important aid furnished to the chemist by physical methods since the time of Dulong and Petit. In Meyer's laboratory Auwers devised a piece of apparatus for conveniently determining the freezing-point and made a series of observations. Other forms of apparatus for use in the laboratory have been described by Hollemann, Hentschel, Arrhenius, Beckmann, Eykman, Fabinyi and Klobukow. Of these, the patterns due to Eykman and Beckmann have found most acceptance; indeed the form devised by the latter, its main feature being a specially constructed thermometer, is now regarded as a necessary part of the equipment of any well-appointed laboratory. For very delicate measurements, Raoult, Pickering, Loomis, H. C. Jones, and Lewis, have also described pieces of apparatus.

Attempts have been made to adapt the various modes of estimating vapour-pressure to the determination of molecular weights. In 1889 Beckmann tried to suitably modify the statical method, but his work, as well as that of Tamman and Loeb, clearly demonstrates the difficulties which stand in the way of its adoption in the laboratory. Methods which consist in estimating the weight of vapour given off from solvent and solution have been used by Tamman, by Walker at Ostwald's suggestion, by Will and Bredig, and by Beckmann. They can only be satisfactorily used with solutions of medium concentration. Ostwald has proposed the employment of a hygrometer, and Charpy has made some measurements with this instrument. By far the most convenient apparatus is based upon the dynamical or boiling-point method of estimating vapour-pressure. Many of Raoult's results were taken in this way, and in 1889 and 1891 Beckmann described two handy pieces of apparatus which admit of the determination of molecular weights from the boiling-point of solutions with almost as much ease and precision as from the freezing-point. Wylie and Sakurai have also described similar methods.

In what has been said regarding the boiling-point of solutions it has been presupposed that, as compared with

the solvent, the dissolved substance is practically non-volatile. Planck (1888) has theoretically investigated the more general case of solutions of volatile substances, and Winkelmann (1890) has experimentally verified Planck's conclusions. Nernst has more recently taken up this question, and shown that as a consequence of the new theory it is possible to obtain evidence regarding the molecular weight of a volatile substance from the boiling-point of its solutions. Because, if the molecular weight of the dissolved substance be the same in the solution as in the state of vapour, the partial pressure of its vapour should be proportional to the strength of the solution, and the rise or fall which it produces in the boiling-point of the solvent should be proportional to the concentration ; whereas, if the molecular weights differ in the two states, there should be no proportionality.

Observations on osmotic pressure may also be employed to ascertain molecular weights, although the methods are in no case so convenient as those just alluded to. The most suitable of the indirect methods are based upon the use of vegetable cells, or red blood corpuscles (see vol. i., pp. 19-22). By the former, in 1888, De Vries concluded that of the formulæ which had been proposed for raffinose, the C_{18}-formula was correct. This conclusion has since been confirmed by purely chemical evidence. The plasmolytic methods are of interest as they show how the microscope may be employed to determine molecular weights. They can only be used, however, with solutions in which protoplasm can live.

In 1889, Ladenburg announced that he was perfecting a method of estimating molecular weights from direct observations on osmotic pressure, but as yet no further details have been published.

The general kinetic theory of solutions.—Not only has the new theory of solutions proved itself a powerful stimulus to the development of such a practical subject as the determination of molecular weights, but it has also given rise to several important speculations regarding the general kinetic theory of solutions. These have, in the main, confirmed the hypothesis of the new theory.

On the assumption that dissolved substances exist as simple molecules, and that the energy of translation of the molecules is the same as in the case of a gas at the same temperature, Boltzmann, Lorentz, and Riecke (1890-91), each starting from a different point of view, have found, by means of purely mechanical principles, that the pressure of dissolved molecules should be equal to the corresponding gaseous pressure. From thermodynamical considerations, Planck and Duhem arrive at results which, in the main, support the same conclusion, and van der Waals' complex equation for a mixture of two substances, in the case of a dilute solution, simplifies down to the gas-equation.

Dissolution and evaporation.—As consequences of the new theory of solutions may be classed the views propounded by Nernst regarding the nature of the process of dissolution, and the results which have been derived therefrom. The analogy between dissolution and evaporation was pointed out by Dossios as long agó as 1867, and from time to time it has been employed by writers on solution. The conception of osmotic pressure for the first time rendered this analogy definite. According to Nernst (1889), we may imagine that when a substance is in contact with a solvent an expansive force is at work whereby molecules pass into the solvent until the pressure they exert—the osmotic pressure—reaches a definite value, which we may term the *solution-pressure.* The process is thus taken to be strictly comparable with that whereby a solid or a liquid evaporates into a gas until its partial pressure reaches a definite limiting value, the vapour-pressure.

Diminution of the solubility.—Of the various consequences which flow from this conception of the nature of dissolution, the effect of a dissolved substance on the solubility of a liquid may be discussed here. If the analogy between dissolution and evaporation be valid, since the vapour-pressure of a solvent is diminished by the presence of a non-volatile dissolved substance, the solution-pressure and hence the solubility of a liquid A in a solvent B should be diminished by the presence in A of a substance insoluble

in B. Moreover, the diminution in solubility should follow the same law as the diminution in vapour-pressure. Thus, to take an example, using water as solvent, if L be solubility of ether, and L′ the solubility of ether containing a dissolved substance insoluble in water, then in all the equations we have discussed dealing with the vapour-pressures of solutions, we may substitute (L-L′)/L′ for $(p\text{-}p')/p'$, and hence from (3) vol. i., p. 411, we may obtain

$$(L\text{-}L')/L' = n/N \quad (5).$$

Consequently, the relative diminution in the solubility of the ether should be the same as the ratio of the number of molecules of dissolved substance to the number of molecules of ether present in the solution. In 1890 Nernst gave a thermodynamical proof of the above equation; experiments to test its validity, although easy to perform, are not very numerous, however, on account of the difficulty of obtaining suitable solvents. The solvent B must be practically insoluble in the liquid A. In his experiments Nernst used water as solvent and found the solubility of valeric acid, ether, and ethyl acetate, containing different dissolved substances. So far as these experiments go they accord with equation (5).

We have here, therefore, another indirect mode of proving that the gas-equation is applicable to dilute solutions and another mode of estimating molecular weights. Quite recently (1894) Küster has attempted to make such measurements on solubility the basis of a laboratory method of determining molecular weights.

Osmotic pressure and the partition-coefficient.—The analogy between dissolution and evaporation leads to another and indeed the most direct indication that osmotic pressure is independent of the nature of the solvent. Berthelot and Jungfleisch found in 1872 that the partition-coefficient of a substance between two mutually insoluble solvents was independent of the amount of dissolved substance. Thus at 15°, if iodine be shaken with water and carbon bisulphide, the ratio of the concentration of the iodine in the bisulphide, to that in the water, is 410, no matter how much iodine be employed. This result leads

to the conclusion that if iodine were gasified it would obey Henry's law, its solubility in either solvent being proportional to the gaseous pressure. Now van't Hoff has shown that for a substance obeying Henry's law the osmotic pressure is equal to the corresponding gaseous pressure, hence it follows from the constancy of the partition-coefficient that if one gram of iodine be dissolved in the same volume of water or carbon bisulphide, the osmotic pressure will be the same in each case (Nernst, 1891).

Correlation of the different modes of proving the validity of the gas-equation.—We have now dealt in detail with five different modes of determining molecular weights and of proving that the gas-equation applies to dilute solutions. Why each of these methods leads to the same result depends upon the fact already stated, that the properties to which they refer are correlated properties. A simple mode of showing the reason for the coincidence in the conclusions to which they lead, and of indicating the nature of their correlation, has been given by Nernst, who has called attention to the fact that they are all concerned with the separation of solvent and dissolved substance. In the case of osmotic pressure this separation is effected by directly forcing the solvent through a semi-permeable membrane, in the case of the freezing-point the solvent is made to crystallise out, in the case of vapour-pressure and boiling-point it is made to volatilise, and finally in the solubility-method, a solvent A is made to separate from a liquid B by dissolving in B a substance insoluble in A. Each method furnishes a means of ascertaining the work required to bring about this separation, and hence each is but a different mode of attacking the same problem.

In the above methods the solvent is made to separate from the solution. It is of course possible to devise parallel methods in which the dissolved substance is made to separate—by crystallisation, evaporation, etc.—and already van't Hoff, Nernst, Deventer and Stadt, and others, have laid the foundation of the development of this side of the question.

J. W. Rodger.

(*To be continued.*)

RECENT RESEARCHES IN THERMAL META-MORPHISM.

PART I.

FEW questions, if any, on the physical side of geology have attracted more attention than those connected with the metamorphism of rock-masses ; and, in view of the vast areas occupied in many parts of the world by rocks which either are, or have been considered, metamorphic rocks, it cannot be said that the importance of the subject has been over-rated. Most of our geological text-books divide metamorphism into "local" or "contact," observed in the vicinity of intruded igneous rocks, and "regional" or "normal," more widely distributed and ascribed rather vaguely to various causes. When we come to discuss the probable conditions under which extensive transformations of rock-masses have been effected, this division does not seem to be a philosophical one. The two causes to which geologists have been led in seeking the explanation of the most striking phenomena of metamorphism are high temperature and mechanical force, and the true distinction therefore seems to be between *thermal* and *dynamic* metamorphism. As regards structural and, to a less extent, mineralogical changes, the two sets of phenomena are found to be in great part distinct : there must arise, however, cases in which the discrimination presents difficulties, and cases in which the effects of the two causes have been impressed simultaneously or successively upon the rocks of the same area.

"Contact-metamorphism," due to the heat of an intruded igneous magma, must stand as the typical case of thermal metamorphism, in the sense of offering the simplest and least ambiguous examples for investigation ; but it is not the only case. In the rise of the isogeotherms, as pictured by Babbage, Mellard, Reade and others, extensive tracts of rocks once cool may be directly invaded by the

internal heat of the earth, and thermal metamorphism thus produced on a regional scale. Further, the mechanical generation of heat in connection with crust-movements must give rise to phenomena of thermal metamorphism, which we may expect to find often complicated by purely dynamic effects.

In this country a considerable amount of study has been devoted to thermal metamorphism in the last twenty years, the lead being taken by Allport in Cornwall, and Clifton Ward in the Lake District. Among more recent contributions may be mentioned those of Miss Gardiner (1) on the New Galloway district, Barrow (2) on the South-eastern Highlands of Scotland, and Harker and Marr (3) on the rocks surrounding the Shap granite. On the continent Rosenbusch has rendered classic the district of the Steiger Schiefer in the Vosges, Lossen the Harz Mountains, Brögger and afterwards Lang the neighbourhood of Christiania, and Barrois the west of Brittany. As preliminary to some more general remarks on thermal metamorphism, I select some recent researches in Saxony, which illustrate characteristic phenomena in rocks belonging to various lithological types, and are worthy of being better known by English geologists.

The metamorphism produced by the intrusion of the great mass of the Meissen syenite was described by the officers of the Saxon Geological Survey a few years ago. Some of the features, as noticed by Sauer (4) in the memoir accompanying sheet 48 of the map, are well worthy of notice. The aureole of metamorphism has a breadth of about two kilometers, or in places where the syenite underlies the strata at a low angle as much as four kilometers. The rocks affected belong to the Ordovician and Silurian formations. They are for the most part ordinary clay-slates with disseminated carbonaceous matter; but these sometimes pass, on the one hand, into fine-grained siliceous slates (Kieselschiefer and Adinolschiefer), and, on the other hand, into bands of greywacke and beds of quartzite. There are also basic volcanic rocks ("diabase-tuffs" and "diabases"), and, associated with these, beds of pure limestone.

The clay-slates and cleaved greywackes are converted in the neighbourhood of the intrusion into rocks composed chiefly of andalusite, biotite, and quartz, with or without a schistose character, or into sillimanite-bearing quartz-biotite-schists. The andalusite, in crystals up to 1 cm. long, is very abundant, and, as usual, crowded with inclusions of other minerals, such as quartz, biotite, and magnetite. Muscovite and felspar have also been formed, and of these too the larger crystals are full of inclusions. The sillimanite occurs in the usual crowded aggregates of needles, but it is curious that these occur even more in the biotite-flakes than in the quartz. The minute rutile-needles, which are abundant in the original clay-slates, have disappeared in the metamorphism, the titanic acid being taken up by the new-formed biotite.

Outside the zone of most highly altered rocks is a zone of "knotted" or "spotted" mica-schists (Knotenglimmer-schiefer), and a point of special interest is the discovery that in a large part of these rocks the "knots" are really crystals of cordierite. In the siliceous and alum-slates, which in their original state contained a quantity of free hydrous silicate of alumina, the alteration extends to a greater distance from the syenite, the characteristic product being chiastolite in crystals 1 cm. long.

The adinole-like rocks are metamorphosed into quartzite-schists, and the original quartzites appear to have been recrystallised near the intrusion. The quartz in the metamorphosed rocks is free from fluid-pores.

The basic tuffs have given rise to hornblende-schists and especially anthophyllite-schists, the augite and its decomposition-products being converted into hornblende or actinolite or to a great extent into the rhombic amphibole anthophyllite, while the felspars are recrystallised. The associated limestones, originally fine-textured to compact, become usually rather coarse-grained marbles; but the thin-bedded alternations of limestone with tuff give rise in the immediate neighbourhood of the syenite to more complex mineral-aggregates similar to those well known at Monzoni in the Tyrol. The minerals produced include garnet, ido-

crase, epidote, zoisite, augite (malacolite), hornblende, biotite, anthophyllite, and cordierite.

Farther south, on sheet 64 of the map, older rocks, including phyllites, mica-schists and gneisses, come within the influence of the syenite. The phenomena have been briefly described by Dalmer (5). The phyllites are converted into "Fruchtschiefer" like those known in the Erzgebirge. The little concretionary patches consist of a dark green substance containing a high percentage of alumina and ferrous oxide and ten per cent. of water. The chief effect of metamorphism in the mica-schists is the production of andalusite, while the gneisses show no appreciable change.

The above areas lie to the west and north-west of Dresden. The district to the south-east of that city, still on the left bank of the Elbe, exhibits features of at least equal interest, which have recently been described in several memoirs by Beck (6). The chief intrusive masses with which the metamorphism is connected are the Lausitz granite and a syenite similar to those of Meissen and Dresden. The rocks affected range from pre-Cambrian to Devonian, and belong to very various petrographical types.

The pre-Cambrian phyllites show the following successive stages of alteration, as they approach the granite: (i.) "Fruchtschiefer" with unaltered matrix, (ii.) "Fruchtschiefer" with recrystallised matrix, (iii.) schistose micaceous rocks, and (iv.) andalusite-mica rocks. The changes involved are the gradual disappearance of the chlorite, with concurrent development of brown mica and andalusite, the recrystallisation of the quartz with increasing coarseness of grain, and the production of what the author styles typical "contact-structure," *i.e.*, an interlocking of the irregularly polygonal grains of such newly-formed minerals as quartz and felspar. Precisely similar phenomena were previously described by Dalmer in the Western Erzgebirge. Some chlorite-gneisses associated with the phyllites are converted into biotite-gneisses, while certain felspathic quartzites also show a production of biotite and a recrystallisation of quartz and felspar in a clear mosaic ("contact-structure").

The Ordovician and Silurian rocks of the district have a great variety of characters. The clay-slates pass through the stages of "Knotenschiefer," "Knotenglimmerschiefer," and "Hornfels" with the production of biotite, quartz, muscovite, etc., but cordierite is uniformly produced instead of andalusite. Pyrite is replaced by pyrrhotite. The rutile-microlites of the original slates are recrystallised as larger grains of rutile or locally converted into little crystals of brookite. The siliceous rocks ("Kieselschiefer," etc.), some of which seem to be radiolarian cherts, are converted into quartzites; graphite, up to as much as two per cent., representing the original carbonaceous matter. Other minerals produced are biotite, chiastolite, and pyrrhotite. The grey-wackes are recrystallised as a mixture of quartz, biotite, muscovite, plagioclase, etc., while their clastic origin is still indicated by angular fragments of quartz which keep their original outline, but are seen to resolve in polarised light into a mosaic of smaller grains.

The calcareous rocks give rise to an important observation which is probably of general application. The pure limestones are simply recrystallised as marble, while the impure ones lose their carbonic acid, and pass into rocks consisting mainly of lime-bearing silicates. The characteristic minerals here are garnet and augite: actinolite, epidote, zoisite, etc., also occur, but wollastonite, idocrase and scapolite are not found. The absence of wollastonite seems rather remarkable, since the limestones analysed are not magnesian. In other places more complex results have been produced by an impregnation of the limestones with ore-material, probably concurrent with the metamorphism.

Certain "diabases" (apparently andesitic lavas) show considerable metamorphism, the most characteristic feature being the amphibolisation or uralitisation of the augite, which process is seen in various stages. Besides the light green hornblende, which is the most prominent constituent of the altered rocks, occur subordinate biotite, unchanged augite, sphene, iron-ores, etc., while the general ground-mass of the rock is a mosaic of clear granules of plagioclase. The characteristic pyrrhotite is here wanting. Tuffs asso-

ciated with the lavas show, on the whole, similar changes. Anthophyllite sometimes takes the place of hornblende. There occur also banded schists, in which alternate layers are rich in green hornblende and colourless augite, respectively. The latter mineral is a malacolite variety, and is clearly a metamorphic product.

The metamorphism of the Devonian strata, consisting of greywackes and conglomerates, also presents some points of interest. In particular, the metamorphosed pebbles still retain their outlines in their metamorphosed matrix.

These observations, made in the neighbourhood of the large intrusive masses of Saxony, have been cited because they raise several points of general interest. Without mentioning in detail the results obtained in numerous other areas of metamorphism, we may now go on to notice these and some other points in a more general manner. Since the phenomena of metamorphism depend in the first place upon the nature of the rocks metamorphosed, the remarks to be made will be most conveniently grouped in that way.

Beginning with the *argillaceous* sediments, which have, perhaps, received more attention than other types of rocks in connection with thermal metamorphism, we may first note as highly characteristic the production in the altered strata of anhydrous aluminous silicates, including under that name the simple silicates of alumina and some more complex minerals containing a large proportion of that base. Since these aluminous silicates are, with unimportant exceptions, absent from all rocks known to have been formed from igneous fusion, they may perhaps afford a useful criterion in some cases. Indeed it would scarcely be too much to say that, when a gneissic or other rock of problematical origin is found to contain abundant andalusite or some allied mineral, there is a considerable presumption in favour of regarding the rock as a metamorphosed argillaceous sediment. In the composition of a normal igneous rock there is a sufficiency of alkalies, lime, and other bases, and of silica, to enable all the alumina to be built up into felspars, pyroxenes, and other common "rock-forming"

minerals. But in the processes by which such a rock furnishes the material for sediments, chemical as well as mechanical degradation plays a part; and, in particular, the soluble become separated from the insoluble products of alteration; the alumina, in whatever combination, remaining among the latter, and so becoming relatively more abundant. Recrystallisation of the derived sediment by thermal metamorphism must therefore give rise to new combinations, including some mineral rich in alumina.

Andalusite and its variety chiastolite have long been known as characteristic metamorphic products in altered shales and slates; but it has only recently been recognised that cordierite plays in some districts a similar part. The widespread occurrence of this mineral as a "contact" product was first made known from the Saxon area described above, but it is found under the same conditions in other parts of the world, and it is possible that further examination may discover it in some districts where it has been overlooked.

In Japan, cordierite is known in several localities. It was noted by Hussak in 1883 from the Asama-yama district, but recently a more remarkable occurrence has been described by Kikuchi (7) from the Waterase-gawa. The mineral occurs there as a "contact-mineral" in dark Palæozoic slates, and is remarkable not only for the large size and frequent freshness of its crystals, but also for their peculiar structure. Besides the usual polysynthetic twinning, giving a hexagonal cross-section with division into six sectors, each crystal shows an inner and an outer portion. The division between the two portions shows as a hexagon in transverse sections and as two diagonal lines in longitudinal sections. These and the junctions of the several twinned individuals are marked by accumulations of inclusions, mostly of carbonaceous matter. The mineral thus presents a distinct variety of cordierite corresponding with the chiastolite variety of andalusite.

Some approach to this structure is seen in cordierite in the metamorphosed Skiddaw Slates of the Caldew Valley in Cumberland (8), but the crystal-grains are there much

smaller, less perfect, and less fresh than in the Japanese occurrence. Hutchings (9) has recently identified as cordierite the essential mineral of the "spots" in the metamorphosed Coniston Flags near the Shap granite, and remarks its close resemblance to the occurrence noted by Beck in the Elbe Valley in Saxony. The grains are small, rounded, and full of inclusions, and do not show the polysynthetic twinning so characteristic of better developed crystals of cordierite. In view of this discovery and that near the Skiddaw granite, it seems probable that cordierite may be found in other areas of "spotted" slates in this country.

The curious spotted or nodular rocks (Fleckschiefer, Knotenschiefer, etc.), which are so common in districts of metamorphosed slates, still present some difficult problems. In one type the spots, as described by Rosenbusch and others, are due merely to local aggregation of the carbonaceous or other pigment which was originally disseminated through the general mass. Rocks of this kind are found in the outer border of many areas of thermal metamorphism, and usually show little or no important recrystallisation. Quite distinct from these are spotted rocks showing more advanced alteration, in which the spots are little patches relatively free from the biotite or other conspicuous secondary mineral produced in the metamorphism. Examined by optical tests in thin slices, these spots often show a distinct crystalline reaction, and are found to be really imperfectly formed crystals, often further obscured by numerous foreign inclusions. In some cases the constituent mineral of the spots has been found to be andalusite; in others, as already mentioned, it is cordierite; and it is possible that other aluminous silicates may figure in some other cases. It is to be noticed that in the most advanced stages of metamorphism spotted and nodular structures of all kinds have usually disappeared altogether, and the same is often true of the more marked foliation and parallel-structures frequently observed in the less metamorphosed strata. The rocks at the inner border of an aureole of contact metamorphism (the Hornfels of German writers) commonly show in thin slices a mosaic of irregular polygonal grains

due to complete recrystallisation of the rock-mass: this is what Beck terms "typical contact-structure".

Numerous instances are known in which chiastolite is produced in relatively large crystals in the early stages of metamorphism, but gives place in the more altered rocks to andalusite, usually in smaller and less perfect crystals. Again, sillimanite or fibrolite, dimorphous with andalusite, occurs abundantly in some very highly metamorphosed slates, flags and grits. It is embedded as bundles of parallel or sub-parallel needles in grains and patches of quartz, to which it imparts a peculiar opacity and silky lustre ("quartz sillimanitisé" or Faserkiesel of some continental geologists), or it occurs in similar fashion in the interior of flakes of muscovite or sometimes of biotite. It has been described by Barrois in Brittany, and also occurs in abundance in the highly metamorphosed rocks described by Miss Gardiner (1) near New Galloway and by Barrow in Forfarshire (2). In the latter area occurs still another form of the pure silicate of alumina, *viz.*, disthene or cyanite.

Such facts as these raise an interesting question: Can we to any extent specify the conditions which determine what particular aluminous silicate shall be formed in a given case? The minerals produced during metamorphism must, of course, depend in the first place upon the nature of the rocks affected; but in the group of minerals considered some (chiastolite, andalusite, cyanite, sillimanite) have identically the same chemical composition, and some others do not differ very greatly from one another in this respect. We are therefore led to infer that physical conditions, and especially the temperature attained during the metamorphism, must be a considerable factor in determining the formation of one or other of the allied minerals. Barrow's work in Forfarshire has an important bearing on this question. There can be little doubt that the striking features of that area are due, in the main, to thermal metamorphism on something approaching a regional scale, but the phenomena of extreme metamorphism are localised in connection with certain intrusions of gneiss and pegmatite. In the altered rocks, which have undoubtedly once been

sediments, the author named has mapped three zones of metamorphism, characterised respectively by staurolite, cyanite, and sillimanite. Followed along its strike, a bed which in the outer zone is a staurolite-schist, becomes in the intermediate zone a cyanite-gneiss, and in the inner one a coarse sillimanite-gneiss. These differences seem then to be due to differences of temperature; and, if it is permissible to press the result to its logical conclusion, the line dividing the cyanite-zone from the sillimanite-zone represents what was an isothermal line during the metamorphism. It is even possible to form some idea of the temperature to which this line corresponds. Vernadsky's experiments have shown that cyanite can be converted into sillimanite by raising it to 1320° to 1380°C.: allowing for the great pressure under which the metamorphism was doubtless effected, we may conclude that the temperature reached at the outer edge of the sillimanite-bearing zone was considerably higher than this figure.

In numerous districts geologists, in studying the "aureole" of metamorphism around a large plutonic mass, have divided it into three or four successive zones or rings, the stages of advancing metamorphism being marked sometimes by the incoming of some special mineral, but also by the appearance and disappearance of spotted, foliated, or other structures in the rocks. The results in different districts do not always bear very close comparison. Such observations as Barrow's, however, would seem to import an element of precision into the subdivision of a metamorphic aureole, and it is therefore very desirable that the interpretation of them should be confirmed by similar study in other regions. Other test-minerals might also be selected: the trimorphous minerals consisting of titanic acid at once suggest themselves. Minute needles of rutile seem to be almost universally present in ordinary clay-slates. In the metamorphism of such rocks the titanic acid is often taken up by new-formed brown mica, or sometimes by micaceous ilmenite or granular sphene; but in some circumstances the little needles are simply recrystallised as stouter prisms of rutile. Again the rutile-needles may be transformed into

little crystals of anatase, as found by Hutchings in the Coniston Flags near the Shap granite (3), or of brookite, as described by Beck (6), (10) in the slates of the Elbe Valley. These latter, purely paramorphic, changes, however, seem to occur locally and somewhat capriciously, so that it is not yet possible to refer the production of one or other of the three forms of titanic acid solely to the effect of higher or lower temperature of metamorphism. Further study in this line may perhaps lead to some more definite conclusion.

An interesting result of such researches as those of Miss Gardiner and of Barrow in Scotland is the clear evidence that thermal metamorphism of sedimentary strata may give rise to thoroughly typical gneisses, some characterised by such minerals as garnet, sillimanite, etc., but also in many examples rich in felspar. The abundant occurrence of felspars of various kinds in thermally metamorphosed sediments is now established by the observations of many petrologists. The apparent reluctance with which some have admitted it is perhaps partly due to the difficulty of identifying the minerals when they occur in a fine-grained mosaic. Such varieties as orthoclase, albite, and oligoclase naturally occur plentifully in the metamorphism of such slates, flags, and greywackes as had not been greatly impoverished in alkalies in the processes by which they were derived from felspathic crystalline rocks; and in these the aluminous silicates may be only feebly represented. Facts drawn from the study of thermal metamorphism cannot fail to have an important bearing on the problem of the gneisses and other crystalline rocks which constitute the much-debated "Grundgebirge" of large tracts of country in Europe and elsewhere. It is becoming increasingly manifest that under such a name as "gneiss" have been included rocks having quite diverse modes of origin. Some are doubtless igneous rocks, with structures either original or impressed on them by dynamic metamorphism; but others are to be regarded as metamorphosed stratified rocks of various types. It has already been remarked that the occurrence of special characteristic minerals may throw light on the origin of such rocks in some cases. Chemical analyses may

be at least equally significant. On the ground of chemical composition alone, Rosenbusch (11) has claimed as metamorphosed sediments various gneisses in Sweden, in the Black Forest, in the Odenwald, in Bavaria, etc., as well as certain hornblende-schists and allied rocks; while he has shown that other gneisses in Sweden, in Saxony, in the Vosges, etc., give chemical analyses, which affords strong presumption of their true igneous origin. Under the latter head he also places the Saxon granulites, various hälleflintas and eurites from Sweden, and pyroxene-granulites, eclogites, chlorite-schists, and talc-schists from various localities. The day of sweeping theories of the origin of gneissic and foliated crystalline rocks in general is past, and it is very generally admitted that each area must be treated as offering a separate problem in itself. We have yet to learn to what extent the hypothesis of thermally metamorphosed sediments is applicable to large tracts of crystalline rocks, but the present tendency of opinion, while leaving room for the dynamic theories of the school of Heim and Lehmann, still points to a partial vindication of the early Lyellian doctrine of metamorphism.

Passing on to the *arenaceous* rocks, we may remark first that different types which would be roughly grouped under this head give rise by metamorphism to widely different products. In the metamorphism of a pure quartz-sandstone there can, in the nature of the case, be no very sharply defined stages. The rock is not capable of any transformation short of complete recrystallisation, and so it will be practically unaltered except where a certain high temperature has been reached during the metamorphism. Some minor changes should, perhaps, be excepted, such as the expulsion of water from the fluid-pores of the quartz, an effect noted by Sorby at Salisbury Crags, Edinburgh.

The quartzite which results from the complete recrystallisation of a sandstone is, in general, easily distinguished, at least in microscopic preparations, from a quartzite produced by the infiltration of a siliceous cement. In the latter case the difference between the original clastic grains and the new-formed quartz-cement is clearly accentuated, while the

former shows in thin slices a uniform mosaic structure. Local conversion of sandstones into quartzites of this type is frequently seen where the strata abut upon any considerable body of intrusive rock. In many cases the metamorphism is further made evident by the production of special accessory minerals arising from impurities (decomposition-products, etc.) in the original sandstone. Near the Shap granite (3) the grits in the Coniston Flag group are converted into quartzites containing numerous granules of pyroxene, probably formed from kaolin and calcite in the strata metamorphosed. The quartzite of Clova, in Forfarshire (2), contains a curious green mica, and other accessory minerals are found in other examples.

An interesting account has been given by Horne (12) of the changes produced in the radiolarian chert in the Arenig beds of the south of Scotland near the large granite mass of Loch Doon. At about a mile from the contact the chert begins to take on a granulitic structure, which becomes more pronounced and coarser, while brown mica begins to be developed. At half a mile from the granite, where this change is well-marked, the traces of radiolaria can still be detected. Near the contact the chert is completely recrystallised, a curious feature being the numerous perfectly rounded inclusions of biotite contained in the quartz-grains, which otherwise constitute the whole of the metamorphosed rock. The carbonaceous matter, which in the analogous rocks described by Beck gives rise to graphite, is here absent. In the Devonian and Carboniferous "Kieselschiefer," metamorphism by the Brocken granite has apparently expelled the carbonaceous matter almost completely (Lossen, 1888).

More impure gritty rocks give rise by metamorphism to less simple products. Silicates of alumina, garnet, micas, etc., may be extensively produced, and the resemblance to a quartzite is lost. Sillimanite is very plentiful in some highly metamorphosed rocks, such as those of Guéméné in Brittany, described by Barrois (1884). In coarse sediments of composite nature evidence of their clastic origin may be preserved after very considerable modification, as we have

seen in the Saxon greywackes and conglomerates. Recrystallised felspars play a more or less important part in many highly metamorphosed arenaceous strata, which are sometimes typical gneissic rocks of somewhat coarse texture. The Silurian gneisses of the New Galloway area (1) have been in part grits and flags rather than shales.

In his valuable report on the igneous rocks of Arkansas, the late J. F. Williams (13) described some very curious contact-phenomena in the siliceous rocks of Magnet Cove, a district which has long been known to mineralogists as the locality of several interesting mineral species and varieties. The strata in question are the well-known novaculites or whetstones and the sandstones associated with them, and the special minerals developed are ascribed to the invasion of a group of elæolite-syenites and allied igneous rocks. The most interesting mineral is brookite, which occurs in brilliant black crystals coating the faces of certain large crystals of quartz. It is of peculiar habit and characters, and was originally described under the name arkansite. Brookite resulting from the metamorphism of rutile-needles has been noticed elsewhere, but the conditions of its occurrence at Magnet Cove seem to be different. Williams states that the unaltered strata are practically free from titaniferous material, and he supposes the titanic acid which builds the brookite crystals to have been derived from the syenite. Good crystals of rutile have also been formed, besides rutile paramorphic after brookite. Crystals and rosette-like groups of hæmatite, which occur sparingly, are also probably titaniferous. On the question here raised, of an actual transference of material from an invading magma to the adjacent rocks, some remarks will be made later.

Some of the most instructive features of thermal metamorphism are found in connection with *calcareous* strata. Pure limestones and dolomite-rocks, which have been subjected to a sufficiently elevated temperature, are always found to have recrystallised without chemical change. The carbonates are not decomposed by heat alone. The resulting crystalline marbles have commonly lost all trace of

original structures, fossils, etc., and may closely resemble the crystalline limestones produced from similar calcareous sediments by aqueous agency alone. In many cases, however, the crystalline limestones and dolomite-rocks which owe their characters to thermal metamorphism are readily distinguished by the occurrence in them of special accessory minerals arising from non-calcareous impurities in the original strata. The most significant of these special minerals are various silicates rich in lime or sometimes in lime and magnesia. Some of them, such as anorthite and the various augites and hornblendes, are well-known minerals of igneous rocks; others, such as wollastonite, zoisite, idocrase, and lime-garnets, are rare or unknown as constituents of rocks formed from igneous magmas.

In a tolerably pure limestone these accessory metamorphic minerals are formed sporadically or often in nests or streaks marking the places where siliceous or argillaceous impurities were collected in nodules or bands. Two or more minerals are often associated with a definite arrangement. Thus, in certain beds of the Coniston Limestone metamorphosed by the Shap granite (3) occur nests consisting of stellate groups of idocrase crystals surrounded by a shell of felspar, which is again bordered by clusters of pyroxene-granules passing into the matrix of crystalline calcite. Attention to the mode of occurrence of the accessory minerals is sometimes necessary to avoid mistakes as to the origin of a rock. The well-known Tiree limestone is an example. This is a rather finely crystalline marble containing abundant grains of salite with some felspar, sphene and other characteristic metamorphic minerals; but a closer scrutiny makes it clear that these minerals have not been formed in place, but occur here as detrital elements. The structure of the rock and the interesting effects of pressure on it have been described by Bonney (14). The grains of salite, etc., may have been derived from some pre-existing metamorphosed limestone. Sollas and Cole (15) have suggested that they may represent a volcanic sand mingled with what was once a coral sand.

An interesting contribution to thermal metamorphism is given incidentally by Brögger and Bäckström in their memoir on the minerals of the garnet group (16). Under the head of alkali-garnets they place sodalite and its allies, including the natural ultramarine lasurite, the constitution of which is shown to be analogous to that of the garnets proper. The well-known, but hitherto mysterious, lapis lazuli or Lasurstein is found to be a product of thermal metamorphism in a calcareous rock. It consists of lasurite, blue and white haüyne, diopside, often hornblende and a micaceous mineral, and calcite, with pyrites, etc.

Some interesting phenomena of metamorphism in calcareous rocks have been described by J. F. Williams (13) at Magnet Cove in Arkansas, a district already referred to. The crystalline limestone at that locality contains several special minerals referable to thermal metamorphism, the most important being lime-bearing silicates. These include crystals of perofskite or dysanalyte, idocrase, and the lime-magnesia-olivine monticellite, the conditions resembling in some degree those of the classical district of Monzoni in the Tyrol. Other minerals met with are octahedra of magnetite, masses of apatite-needles, and a green biotite.

The occurrence of dipyre in the metamorphosed limestones of Ariège and the Pyrenees has long been known, and the mode of its occurrence has recently been investigated by Lacroix (17). The lherzolites which are the prevalent igneous rocks of the district appear to have been intruded prior to the deposition of the Upper Jurassic strata, which contain pebbles of the lherzolites. The intrusion of these rocks cannot, therefore, have produced the dipyre which is so frequent in the crystalline limestones of the Upper Jurassic. The limestones of the Middle Lias were, however, cut and metamorphosed by the lherzolites. One type of the metamorphosed rocks consists chiefly of large crystal-plates of dipyre enclosing many flakes of biotite and grains of pyroxene, with abundant sphene and more rarely plagioclase. Another type is very micaceous, irregular crystals of dipyre being disseminated through a mass made up of little flakes of biotite and grains of pyroxene. Rutile,

green spinel, and especially hornblende are locally abundant, and sometimes little crystals of blue tourmaline, an unexpected mineral.

Among other examples we may mention the case, also described by Lacroix, of the Ordovician limestones metamorphosed by the elæolite-syenite of Montreal (18). The limestones become thoroughly crystalline, and their contact with the intrusive rock is marked sometimes by a zone of garnet or cancrinite, sometimes by a granular aggregate of pyroxene, garnet, and wollastonite. The intensity of the metamorphism and the relative proportions of the several minerals formed vary greatly in short distances. There are often patches consisting exclusively of granular silicates, wollastonite, pyroxene, and garnet, sometimes accompanied by felspar and perofskite, intimately associated with the garnet. Here the coming in of such minerals as cancrinite and perofskite (the latter also found near the elæolite-syenites of Arkansas) seems to suggest some degree of connection between the products of metamorphism and the nature of the igneous rock that produced it, which bears on one of the more obscure problems of our subject.

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Alfred Harker.

ON THE KINETIC THEORY OF GASES.[1]

IN the former of the two works which I have taken for the subject of this article, Dr. Watson has given us a clear account of the established results of the kinetic theory. He has also pointed out the nature of some of the difficulties of the theory and given valuable suggestions for their solution.

In Professor Tait's work, as yet unfinished, we have the advantage of seeing the subject from a different point of view. He has ventured a little farther than Dr. Watson into the regions of hypothesis. He has also discussed the difficult subjects of diffusion and viscosity in gases, with which Dr. Watson, following his original plan, has not attempted to deal.

In discussing doubtful points I shall have to refer to the arguments of these writers, and if this is done with some abbreviation, I hope that the reader will supply the defects of my exposition by reference to the originals.

1. The simplest conception of a gas in the kinetic theory is that of a number of perfectly elastic spheres moving within an elastic bounding surface. If p denote pressure per unit of surface, v volume, T the mean kinetic energy of a sphere, the equation $pv = \mathrm{CT}$ (A) is accurate, provided that the spheres be mere particles, having mass but no sensible diameters.

2. The laws of Boyle and Charles, which hold approximately for hydrogen and other permanent gases under ordinary conditions, are concisely expressed by the equation $pv = \mathrm{C}t$ (B), in which t is the absolute temperature. The complete agreement of (A) and (B) suggests the kinetic theory, according to which the molecules of a gas are to be regarded as elastic spheres, and the absolute temperature is measured by their mean kinetic energy.

[1] *A Treatise on the Kinetic Theory of Gases*, by H. W. Watson, D.Sc., F.R.S., second edition, Clarendon Press, Oxford, 1893. "On the Foundation of the Kinetic Theory of Gases," parts i.-iv., by Professor Tait, *Transactions of the Royal Society of Edinburgh*.

3. If the spheres have sensible diameter, collisions will occur among them, and we require a law of distribution of velocities. It will be the well-known exponential or Maxwellian law, ϵ^{-hmu^2} being the chance of velocity u in given direction for a sphere of mass m. And if the spheres have room enough the chances are all independent. The permanence of this distribution, once established, has been proved by several writers.

I shall make some use of the following notation:—

Let V be the velocity of the common centre of gravity of a pair of spheres, ρ their relative velocity. Then if collisions occur at haphazard, it is a property of the sphere that all directions of ρ after collision are equally probable. It is a property of the above distribution of velocities that for given magnitude and direction of V and given magnitude of ρ, whether the pair of spheres collide or not, all directions of ρ are equally probable. This proves the permanence of the distribution if any further proof were wanted. If this law is obeyed, the system is "undisturbed," or, in Professor Tait's language, is in the "special state". If it be not obeyed there is a "disturbance," and the effect of collisions is to remove the disturbance and reduce the system to the "special state".

4. Boltzmann was the first to show that the "special state" is, in the absence of external forces tending to produce disturbance, not only a sufficient, but a necessary, condition for permanence. For that a certain function B is always diminishing with the time, and then only becomes constant when the special state is attained.

5. If all the spheres in a certain volume have given to them, in addition to their molecular velocities in the special or undisturbed state, any common velocity u in any direction, whether u be great or small, the law of uniform distribution in direction of the relative velocities is unaffected. So a gas in this state is undisturbed. Professor Tait calls this "mass motion" of the gas.

6. One of the most useful forms in which to express the permanent character of the motion is the virial equation—

$$\tfrac{3}{2}pv = \Sigma\tfrac{1}{2}m\overline{u^2} + \tfrac{1}{2}\Sigma\Sigma Rr$$

in which R denotes the *repulsive* force, r the distance, be-

tween a pair of spheres, and the summation includes all pairs. If the spheres be material particles without sensible dimensions $\Sigma\Sigma Rr$ vanishes, there being no finite forces. If they be spheres of diameter "ς," and subject to collision, $\Sigma\Sigma Rr$ is proportional to $\Sigma m\overline{u^2}$. For consider two equal spheres A and B, each of mass m. Let ρ be their relative velocity. About the centre of A describe a sphere with radius ς, *i.e.*, twice the radius of A or B. Consider all radii of that sphere which make with ρ angles between θ and $\theta + d\theta$. Let v be the volume in which the spheres, n in number, are supposed to move. Then the chance that the centre of B shall at a given instant be within the element of volume

$$2\pi\varsigma^2 \cos\theta \sin\theta \, d\theta \, \rho dt \text{ is } \frac{2\pi\varsigma^2 \cos\theta \sin\theta \, d\theta \, \rho dt}{v}$$

and if this is the case. A and B will collide within the time dt after the given instant, and the relative velocity in line of centres, namely, $\rho \cos\theta$, will be reversed. Suppose that reversal to be affected by the large finite force $\dfrac{m\rho \cos\theta}{2dt}$ acting on each sphere during the time $2dt$, then R, in the expression Rr, is $\dfrac{m\rho \cos\theta}{2dt}$; also $r = \varsigma$, and the number of pairs of spheres in volume v is $\dfrac{n^2}{2}$; so

$$\begin{aligned}
\Sigma\Sigma Rr &= \frac{n^2}{2v} m \, 2\pi\varsigma^3\overline{\rho^2} \int_0^{\frac{\pi}{2}} \cos^2\theta \sin\theta \, d\theta \\
&= \frac{n^2}{2v} m \tfrac{2}{3}\pi\varsigma^3\overline{\rho^2} \\
&= \frac{n^2}{v} m \tfrac{2}{3}\pi\varsigma^3\overline{u^2},
\end{aligned}$$

because $\overline{\rho^2} = 2\overline{u^2}$, and since $\Sigma m\overline{u^2} = nm\overline{u^2}$ the virial equation becomes

$$3pv = \Sigma m\overline{u^2}\left\{1 + \frac{2n}{3}\frac{\pi\varsigma^3}{v}\right) = \Sigma m\overline{u^2}\left\{1 + \frac{nv'}{v}\right) \text{ if } v' = \tfrac{2}{3}\pi\varsigma^3, \text{ or}$$

four times the volume of one of the n spheres. The effect is that if the gas were compressed at constant temperature, the pressure would increase rather *more rapidly* than it should do were the law $pv = Ct$ accurate. In fact the deviations from the laws of Boyle and Charles that require explanation are in the opposite direction.

7. It is worth while to analyse more closely the conception of an elastic sphere. Our elastic spheres are supposed to have each three degrees of freedom, namely, that of motion of translation in space. The three angular velocities of which each sphere is also capable are to be ignored, because, the spheres being perfectly smooth, these velocities will not be affected by collisions. Again, if our spheres collide with each other, they must separate with their combined kinetic energy unaltered, none of it being dissipated, *i.e.*, converted into heat or vibrations. This is essential if the system is to have the properties of a permanent gas. For otherwise it would change its condition by collisions among the spheres without any influence of external bodies. It would appear then that a sphere, or molecule, such as the theory requires, cannot consist of parts capable of vibrations or other relative motion *inter se*, for otherwise such relative motion would be set up on collision. Therefore, also, that it cannot of itself be either hot or cold, that is, cannot possess the quality temperature as commonly understood. But the whole system of moving spheres has temperature, which, according to the above form of the theory, either is, or is proportional to, the mean kinetic energy of the spheres.

8. It is evident that a body of such very simple structure cannot be expected to discharge all the duties which the chemist requires of the molecules of a gas. Further we know from the phenomena of the spectroscope that the molecules, or atoms, of every gas must be capable of executing vibrations of one or more kinds. It seems then that our system of elastic spheres, though it satisfies with more or less accuracy certain physical properties of permanent gases, which depend on pressure, density and temperature, cannot be expected to explain those phenomena which depend on chemical composition.

9. The following extension of the theory was given originally by Boltzmann, and afterwards in the most general form in the first edition of Dr. Watson's book. For the elastic sphere with its three degrees of freedom we may substitute a molecule of n co-ordinates, q_1, q_2, q_n, and corresponding momenta p_1, p_2, p_n, and in the permanent state

of an infinite number of such molecules sparsely scattered, the chance that any given one shall have co-ordinates between the limits

$$\left.\begin{array}{l} q_1 \text{ and } q_1 + dq_1 \\ q_2 \text{ and } q_2 + dq_2 \\ \text{etc.,} \end{array}\right\} \qquad (a)$$

and momenta between the limits

$$\left.\begin{array}{l} p_1 \text{ and } p_1 + dp_1 \\ p_2 \text{ and } p_2 + dp_2 \\ \text{etc.,} \end{array}\right\} \qquad (b)$$

in proportional to

$$\varepsilon^{-h(\chi+T)}\, dq_1 \ . \ . \ . \ dp_n$$

where χ is the potential, T the kinetic energy in the state (ab). This is the general law of permanence of which the $\varepsilon^{-hmu^2}du$ of elastic spheres is a particular case.

10. We must consider the assumptions on which this theorem rests. Let $f(q_1 \ . \ . \ . \ p_n)\, dq_1 \ . \ . \ . \ dp_n$ be the number of molecules in unit of volume between the limits "a" "b" above, or the chance that a given molecule shall be between those limits, and let $F\,(Q_1 \ . \ . \ . \ P_n)\, dQ_1 \ . \ . \ . \ dP_n$ be the number between the corresponding limits

$$\left.\begin{array}{l} Q_1 \text{ and } Q_1 + dQ_1 \\ Q_2 \text{ and } Q_2 + dQ_2 \\ \text{etc.,} \end{array}\right\} \qquad A$$

and

$$\left.\begin{array}{l} P_1 \text{ and } P_1 + dP_1 \\ P_2 \text{ and } P_2 + dP_2 \\ \text{etc.,} \end{array}\right\} \qquad B$$

The number of pairs, one from each set, is shortly $fFdq_1 \ . \ . \ . \ dP_n$. Let these values of the variables denote the state in which mutual action between the pair commences. Such a pair will in time τ pass by their own mutual actions, uninfluenced by any third molecule, into the state

$$\left.\begin{array}{l} q'_1 \text{ and } q'_1 + dq'_1 \\ q'_2 \text{ and } q'_2 + dq'_2 \\ \text{etc.,} \end{array}\right\} \qquad (a')$$

and

$$\left.\begin{array}{l} p'_1 \text{ and } p'_1 + dp'_1 \\ p'_2 \text{ and } p'_2 + dp'_2 \\ \text{etc.,} \end{array}\right\} \qquad (b')$$

for the first, and

$$\left.\begin{array}{l} Q'_1 \text{ and } Q'_1 + dQ'_1 \\ Q'_2 \text{ and } Q'_2 + dQ'_2 \\ \text{etc.,} \end{array}\right\} \quad A'$$

and

$$\left.\begin{array}{l} P'_1 \text{ and } P'_1 + aP'_1 \\ P'_2 \text{ and } P'_2 + dP'_2 \\ \text{etc.,} \end{array}\right\} \quad B'$$

for the second molecule. Let this be the state when mutual action ceases.

Let $f'dq'_1 \ldots dp'_n$ be the number per unit of volume of molecules in the state a' b', and $F'dQ'_1 \ldots dP'_n$ the number in the state A′ B′. Then shortly $f'F'dq'_1 \ldots dP'_n$ is the number of pairs in the second state.

It is now required that the number of pairs in the first state shall be the same as in the second, that is, $fFdq_1 \ldots dP_n = f'F'dq'_1 \ldots dP'_n$. This has been shown by Boltzmann to be necessary for permanence. But since $q'_1 \ldots P'_n$ are functions of $q_1 \ldots P_n$ and of the time τ and no other quantities whatever, $dq_1 \ldots dP_n = dq'_1 \ldots dP'_n$. Therefore our requirement is satisfied if $fF = f'F'$.

11. If this equation be given only for *particular values* of the variables $q_1 \ldots P'_n$, we cannot draw from it any inference concerning the form of the functions f and F. But if it be satisfied for all possible initial values of the variables $q_1 \ldots P_n$ with the final values $q'_1 \ldots P'_n$ into which they pass under the influence of their mutual action alone, we can infer that the form of the function is $f = F = C\epsilon^{-h(\chi + T)}$.

12. The theorem, as given by Watson, thus depends on three conditions: (1) There must be an infinite number of pairs, including all possible combinations of the variables; and (2) each pair must be uninfluenced by any third molecule during the time of action of the mutual forces between its members. (3) The fact of the near approach of the two molecules to one another, which is implied in their encounter, must not of itself destroy the independence of the chances f and F. This condition involves the property mentioned above that the molecules must be "sparsely scattered". For, if they be crowded, this independence

will be interfered with, as regards at least the velocities of translation (see *post* 34). Condition (2) is assumed to be always satisfied by the molecules of a very rare gas. As the gas becomes denser, we arrive at a state in which no molecule or group of molecules is ever free from the influence of other molecules not belonging to the group. Condition (2) now fails, and the proof, as above given, fails, whether the law continues to hold or not.

13. For the above conditions Boltzmann would substitute the following, namely, that a single system shall, if left to itself, at some time or other pass through every combination of the co-ordinates and velocities which can be reached from its initial state consistently with the conservation of energy. And he would, I think, express the law by saying that on average *the time* during which the co-ordinates and velocities are between the limits "a" and "b" above varies as $\epsilon^{-h(\chi+T)}\, dq_1 \ldots dp_n$. It seems to me to be open to question whether this method would succeed in any case where the other would fail. See an interesting paper of his in the *Philosophical Magazine* for March, 1893, in which he discusses a test case suggested by Lord Kelvin.

14. For the present it is sufficient to point out a particular consequence of the $\epsilon^{-h(\chi+T)}$ distribution, namely, that if a gas consists of stable molecules, each having n co-ordinates, including three of position in space, then $\dfrac{\text{energy of translation}}{\text{whole kinetic energy}} = \dfrac{3}{n}$ on average. (It will be observed that we have at this point introduced a fourth condition besides (1), (2) and (3), namely, that the molecules are to be *stable* systems.) To prove this, consider that T, the kinetic energy of a molecule, is a quadratic function of the velocities of the form $2T = m(\dot{x}^2 + \dot{y}^2 + \dot{z}^2) + a_1\dot{q}_1^{\,2} + b_{12}\,\dot{q}_1\dot{q}_2$ + etc., where x, y, z, are the co-ordinates of position, $q_1 \ldots q_{n-3}$, the remaining co-ordinates, and $\dot{x}\,\dot{y}\,\dot{z}\,\dot{q}_1 \ldots q_{n-3}$, etc., the corresponding velocities, and $a_1\, b_{12}, \ldots$ etc., are generally functions of the co-ordinates.

Now, according to the theorem, if we consider all those molecules for which the co-ordinates $x \ldots q_n \ldots$ etc., have given values, but the velocities may have any values between $+\infty$ and $-\infty$, we can find the mean values of

$\dot{x}_1^2 \dot{y}_1^2 \dot{z}_1^2 \dot{q}_{,1}^2 \dot{q}_2^2$, . . . etc., and also of the products $\dot{q}_1\dot{q}_2$, . . . etc., by integrating the expressions—

$$\varepsilon^{-h(m\dot{x}^2 + m\dot{y}^2 + m\dot{z}^2 + a_1\dot{q}_1^2 + b_{12}\dot{q}_1\dot{q}_2 + \ldots)}\, \dot{x}^2\, d\dot{x}\, d\dot{y}\, d\dot{z}\, d\dot{q}_1, \quad \ldots$$

etc., between the limits $\pm\infty$ for each variable. The result is as follows :—

Form the determinant

$$d = \begin{vmatrix} 2m & & & & & & \\ & 2m & & & & & \\ & & 2m & & & & \\ & & & 2a_1 & b_{12} & \ldots & b_{1\,n-3} \\ & & & b_{12} & 2a_2 & \ldots & b_{2\,n-3} \\ & & & \ldots & \ldots & \ldots & \ldots \\ & & & & & & 2a_{n-3} \end{vmatrix}$$

And let d_{11} d_{12} d_{22}, etc., be its minors. Then we find

$$\overline{\dot{x}^2} = \overline{\dot{y}^2} = \overline{\dot{z}^2} = \frac{d_{11}}{hd} = \frac{d_{22}}{hd} = \frac{d_{33}}{hd}$$

$$\overline{\dot{q}_1^2} = \frac{d_{44}}{hd} \quad \ldots \quad \overline{\dot{q}_1\dot{q}_2} = \frac{d_{45}}{hd}, \text{ etc.,}$$

and therefore $m(\overline{\dot{x}^2 + \dot{y}^2 + \dot{z}^2}) = \frac{3}{2h}$,

also $a_1\overline{\dot{q}_1^2} + b_{12}\overline{\dot{q}_1\dot{q}_2} + \text{etc.}, = \frac{n-3}{2h}$.

$$\text{Therefore } m\,\frac{(\overline{\dot{x}^2} + \overline{\dot{y}^2} + \overline{\dot{z}^2})}{a_1\overline{\dot{q}_1^2} + b_{12}\overline{\dot{q}_1\dot{q}_2} + \text{etc.}} = \frac{3}{n-3}.$$

$$\text{or } m\,\frac{(\overline{\dot{x}^2} + \overline{\dot{y}^2} + \overline{\dot{z}^2})}{2\mathrm{T}} = \frac{3}{n}$$

It is not generally true that $a_1\overline{\dot{q}_1^2} = a_2\overline{\dot{q}_2^2}$, etc. But it is possible, for given values of the co-ordinates, by a linear transformation to reduce the quadratic function to the form $a_1\dot{q}'^2_1 + a_2\dot{q}'^2_2$, etc., containing no products $\dot{q}'\dot{q}'$, and these quantities q' have the property that $a_2\overline{\dot{q}'^2_1} = a_2\overline{\dot{q}'^2_2} =$, etc., if in this form they are of any use. The proof that $\frac{m\,(\overline{\dot{x}^2} + \overline{\dot{y}^2} + \overline{\dot{z}})}{2\mathrm{T}} = \frac{3}{n}$ does not depend on the transformation.

15. It follows from this theorem, as shown first by Maxwell, and as explained in Watson's book, that for a gas

whose molecules are stable and have n degrees of freedom, the ratio of the specific heat at constant pressure to that at constant volume is $\gamma = \frac{n+2}{2}$, or $\frac{n+2+\epsilon}{n+\epsilon}$, where ϵ is a small quantity necessarily positive, that is γ is, or is rather less than, $\frac{n+2}{n}$.

Now it would seem that the molecules (or atoms) of any gas may have, in addition to three degrees of freedom of motion in space, perhaps as many more as the spectrum of the gas contains lines. And if that be the case, $\frac{n+2}{n}$ can on no probable hypothesis be made to represent the observed values of γ.

16. One possible solution of this difficulty is, I think, the following: in deducing the value of γ from the law $\epsilon^{-h(\chi+T)}$ we have assumed that our gas consists only of *stable* molecules, each having the given number of degrees of freedom. Each molecule is then a system which, when left to itself, is in stationary motion. On that hypothesis the virial equation may be put in the form $\frac{3}{2}pv = \frac{3}{n}T + \left[\frac{n-3}{n}T - \frac{1}{2}\Sigma\Sigma Rr\right]$, the terms within the bracket denoting respectively the energy of relative motion of the constituent parts of the molecules, and the mutual forces acting between them, R being now positive when attractive. If the molecule be stable, the two terms within the bracket cancel each other, and we have left $\frac{3}{2}pv = \frac{3}{n}T$, which leads us into difficulties in the form $\gamma = \frac{n+2}{n}$. But if the molecule be unstable $\frac{1}{2}\Sigma\Sigma Rr < \frac{n-3}{n}T$. And therefore $\frac{3}{2}pv > \frac{3}{n}T$. We thus get rid of the particular difficulty, only perhaps to introduce others which may prove as formidable. On this view the gas will consist not of stable molecules only, but of molecules with a considerable admixture of dissociated atoms.

17. The following has also been suggested as a mode of

escape from the difficulty: In the proof of the law $\epsilon^{-h(\chi+T)}$ we assumed that the final co-ordinates and momenta $q'_1 \ldots P'_n$ were functions of the initial co-ordinates and momenta and of no other quantities whatever. But suppose the molecule contains a class of co-ordinates $a_1 \ldots a_r$, with corresponding momenta $\beta_1 \ldots \beta_r$, which are not functions at all of the other co-ordinates and momenta $q_1 \ldots P_n$, but entirely independent of them. If that be so $\chi + T$ in the expression $\epsilon^{-h(\chi+T)}$ is the energy of the qp co-ordinates and momenta only, and will not include the $a\beta$ system. It is true that the $a\beta$ set may be amenable to the same treatment, and we might perhaps prove that for them also the law of distribution is denoted by $\epsilon^{-k(\chi'+T')}$ where χ' is the potential, T' the kinetic energy of the system $a\beta$. But we cannot prove that $h = k$. And so the ratio $\dfrac{m(\dot{x}^2+\dot{y}^2+\dot{z}^2)}{2T}$ need not depend on the number of co-ordinates of the q set and the a set respectively. On this assumption we may make $\dfrac{m(\dot{x}^2+\dot{y}^2+\dot{z}^2)}{2T} \neq \dfrac{3}{n}$, if n and T relate to all the co-ordinates both of the q and a sets.

The hypothesis cannot be unreasonable, because all writers on the kinetic theory of gases have made it once, when they ignore the rotation of their elastic spheres. The three angular velocities of a smooth sphere behave to the translation velocities exactly in the same way as β to p on the above hypothesis. And a system in which the translation velocities are distributed according to the law ϵ^{-hu^2}, and the angular velocities according to ϵ^{-kw^2}, would, if the spheres be smooth, be in equilibrium with $h \neq k$. But the following objection appears to be fatal: If the temperature of the gas depends on the qp system alone, including the velocities of translation, you must on this hypothesis admit that of two equal quantities of gas at the same pressure and temperature, one might contain more energy than the other because it might contain more of the $a\beta$ sort. If temperature depends on both forms of energy, then one must, under some circumstances, be capable of conversion into the other,

because we can raise the temperature by compression alone. So α β, etc., cannot be independent of q p, etc.

18. A step towards the solution of the difficulty, and one which may prove to be important, is taken in Watson's book. He shows, namely, that although the condition for permanence is never attained until the distribution of momenta is according to the law $\varepsilon^{-h(\chi+T)}$, and any disturbance is gradually effaced by encounters between the molecules, the function B always approaching its minimum, yet *the rate at which this process takes place* depends entirely on the nature of the disturbance. If, for instance, the disturbance consists in giving to spheres of mass m greater average energy of translation than those of mass M in a mixture otherwise normal, and under ordinary conditions of pressure, etc., it will be reduced, as Tait has shown, to half of its original amount in a very small fraction of a second. On the other hand, suppose the system of elastic discs described in Watson's book, or a system of elastic spheres, the centre of gravity of each sphere instead of being at the centre of figure, being at a small distance c therefrom. According to the above law the spheres should have energy of rotation on average equal to that of translation. Let the disturbance consist in a small inequality in these average energies. It will be effaced gradually by collisions, but at a rate per unit of time proportional to c^2, and therefore as slowly as we please.

19. This fact alone will not help us much, because we cannot suppose that the gas subjected to experiments for determining γ has not had time during the experiment to attain to its permanent condition. But if it could be shown that any cause exists always increasing the energy of translation and diminishing that of vibration, and so creating a disturbance, while encounters are always effacing the disturbance, the fact that the rate of effacement is very slow may be of great importance. For let $\frac{\delta f}{\delta t}$ be the rate at which the disturbance f is produced by the unknown cause, $\frac{df}{dt}$ the rate at which it is destroyed by encounters.

Then $\frac{df}{dt} = -Kf$, where K is a constant depending on the constitution of the body. And the actual constant value of f will be given by the equation $f = \frac{1}{K}\frac{\delta f}{\delta t}$. And f may be considerable if K be small. But where are we to look for the supposed disturbing cause? Clearly it cannot be looked for in any mutual actions of the gas molecules; for that is always effacing the disturbance already existing. It can be looked for (if at all) only in the action between the gas and external bodies.

20. Now we have appealed to the spectroscope as proving that our molecules have many degrees of freedom, because they, or their constituent atoms, communicate vibrations of many kinds to the luminiferous ether, and are capable of receiving from the ether vibrations of the same kinds. The vibration energy or internal energy of our molecules is thus capable of transmission by what we call radiation from these molecules to external bodies. It is our duty also to believe that some of it is scattered into infinite space and so lost to science, as much as if it ceased to exist, *quia de non apparentibus et non existentibus eadem est ratio.* On the other hand energy of translation is, so far as we know, incapable, while in that form, of being communicated to the ether.

21. If this be true, it follows that some action is always taking place between the molecules of our gas and external bodies. It may be said that this action will not be sensible except at high temperatures, *i.e.*, when the vibrations are very intense. But according to our theory there is even at low temperatures a minority of molecules in the same state of intense vibration which the majority have at high temperatures. And it may be questioned whether we can, as now the fashion is, treat minorities as non-existent. It is improbable indeed that this interaction between gas molecules and external bodies should be sufficiently important to affect the determination of γ. But one consequence must, I think, be admitted, *viz.*, that the law $\epsilon^{-h(\chi+T)}$ can never express with complete accuracy the state of our system. For in our proof of that law we assumed in

condition (2) that our molecules are not interfered with by any body not a member of their own family. But the luminiferous ether is such a body, and therefore, if it have the properties attributed to it of imparting energy to or receiving it from gas molecules, it is itself a part of the material system under discussion, and our conclusions cannot be safe unless we take it into account.

22. It is worth while to note also that no actual gas under experiment can accurately satisfy the condition (2) required by the analysis. For collisions must occur, not only between the molecules of the gas, but between them and the walls of the containing vessel, and in the latter case the condition (2) cannot be satisfied.

23. The solution of this difficulty, when it is discovered, will probably be found intimately connected with the solution of another question in the kinetic theory, namely, What is *the relation between kinetic energy and temperature?* This question has been discussed by Tait, to whose reasoning I shall refer later.

24. *On the deviation of gases from the laws of Boyle and Charles.*

When a gas undergoes compression at constant temperature, it is found that the pressure increases as the volume diminishes *less* rapidly than it would do were the relations accurately expressed by the equation $pv = Ct$. The deviation becomes more marked as the compression increases, and finally a portion of the compressed gas becomes liquid. So that a state is reached in which if any more gas at the same temperature were forced into the same volume, the gas so forced in would all take the liquid form, and the pressure of the still gaseous portion of the substance would be unaltered. The degree of condensation required to produce this result is greater at higher than at lower temperatures; and at a certain temperature, called the *critical temperature* for the substance in question, no degree of condensation converts any of it into liquid, or else the liquid form of the substance is indistinguishable from its vapour. The actual relations between p, v and t for certain gases within certain limits have been determined with great accuracy by the researches of Andrews, and more recently

of Amagat. The results obtained by Amagat for CO_2 are discussed at length by Tait.

25. Our kinetic theory of gases is bound, if it aspires to be complete, to give some account of these phenomena. The first object to which physicists have turned their attention has been to invent a modified form of the virial equation which should express the properties of an actual gas, as determined by Andrews or Amagat, in the same convenient way in which the equation $pv = Ct$ expresses the properties of the ideal perfect gas. For this purpose it is necessary to introduce into the virial equation certain constants. Van der Waals proposed the equation $\left(p + \frac{a}{v^2}\right)(v - \beta) = \frac{1}{3}\Sigma\,(mu^2)$ in which α and β are two disposable constants, but Tait shows that at least three are necessary. Clausius proposed $p = \frac{kt}{v - \beta} - \frac{a}{t\,(v + \alpha)^2}$ in which we have three disposable constants α, β and a (for explanation of k see Tait, part iv., p. 261). Finally Tait gives us $pv = Rt + \frac{C}{v + \gamma} - \frac{A - Rte}{v + \alpha}$. I take his form given, part iv., p. 265, and subsequently modified in notation only. In this Rt is the mean kinetic energy during free path, and, as stated below, $R = \frac{dp}{dt}$.

26. If such an equation can be found, it will be very valuable, perhaps more valuable than a complete kinetic theory. But for a kinetic theory—and that is now our object—we require not only a modified formula accurately expressing the observed facts, but some hypothesis concerning the nature and constitution of gas molecules which shall explain and justify the modified formula. Such a hypothesis Tait has given us, and so far as I know no one else has yet worked out any in detail.

27. His hypothesis is that the molecules behave indeed like conventional elastic spheres when they actually collide with each other, but in addition attract each other with finite force. He assumes further that the effect of that finite force may be approximately represented as follows, dealing with equal spheres only: When two molecules

approaching each other reach the distance a apart (in which a is, of course, greater than s, the diameter of a molecule) an impulse acts in the line of centres increasing the square of the relative velocity u by the constant quantity c^2, supposed to be independent both of u and of the angle which its direction makes with the line of centres at the instant. When the distance is less than a no force acts. This approach is called *an encounter.* If the value of u, and the angle between its direction and the line of centres, be suitably chosen, there will be also an *impact* between the hard nuclei. Whether an impact takes place or not, the molecules will again on separation reach the distance a. And then the square of the relative velocity is again diminished by c^2. Also $c^2 = \frac{2}{\sqrt{h}}e$, e being one of the constants in Tait's virial equation above given.

28. On this hypothesis, if c^2, a and s were all known, the motion of the system for any assigned values of t and v would be determinate, whether our mathematical powers suffice to make all the necessary calculations or not. We should then have only three disposable constants corresponding to c^2 a and s to introduce into the virial equation. So comparing Tait's hypothesis with the practical formula which he derives from it, we seem to have in the latter two constants too many. To adapt the hypothesis to the virial equation, we have to find (1) the modification of the term $\Sigma m\overline{u^2}$, (2) that of $\Sigma\Sigma Rr$. Now $\frac{1}{2}\Sigma m\overline{u^2}$, or the mean kinetic energy, is increased, approximately at least, by adding c^2 for the number of pairs which at any (and therefore every) instant are at less than distance "a" from each other, are in "*entanglement*," as he defines it. The value of $\Sigma m\overline{u^2}$ depends then on the average duration of an entanglement, which he calculates, part iii., p. 1037. The term $\Sigma\Sigma Rr$ has to be modified in two ways. Firstly, calculate the average value of the *attractive* impulse for each encounter, and multiply by the distance "a" at which it takes place. That calculation is performed, pages 1033, 1034. The result will be of the opposite sign to the term $\Sigma m\overline{u^2}$. Secondly, remembering that for some encounters

an impact will take place, for others not, calculate the average value of the *repulsive* impulse per encounter, and multiply by "s" the distance at which it takes place. The result will be of the same sign as $\Sigma m\overline{u^2}$.

29. Tait's reasoning is as follows: The increase of $\Sigma m\overline{u^2}$, depending as it does on the number of molecules in entanglement at any instant, is proportional to the number of encounters. Now for given volume this is proportional to the square of the density, but for a given number of molecules proportional to the density only, and therefore inversely proportional to v. He therefore adds to $\mathrm{R}t$ or E, the term $\frac{\mathrm{C}}{v+\gamma}$ where C is positive, and γ of uncertain sign. In like manner for the two parts of $\Sigma\Sigma\mathrm{R}r$ he writes $\frac{\mathrm{A}}{v+a}$ and $\frac{\mathrm{A}}{v+a}\left(\mathrm{R}t+\frac{\mathrm{C}}{v+\gamma}\right)$ (part iv., p. 265). I see no objection to this reasoning except that it falls short of Professor Tait's own standard of perfection, because it introduces five disposable constants while the theory allows only three. That, of course, does not detract from the value of the formula as an empirical formula, a concession to the weakness of the flesh. And certainly for the values of the constants assumed, p. 271, the formula gives results agreeing remarkably with those of Amagat. Does it not agree with them more closely than is necessary, considering that the calculated results are, in the matter of the constant c^2, only approximate?

30. *The relation between kinetic energy and temperature.* In the original or elastic sphere form of the theory we are compelled to treat kinetic energy of translation as identical with heat, and as a measure of temperature, because it is the only form of energy which we allow our system to possess. When we give our molecules many degrees of freedom, and as many forms of kinetic energy, the question arises, which of the corresponding forms of energy measures temperature? We quieted our consciences for a time by the law $m\frac{(\dot{x}^2+\dot{y}^2+\dot{z}^2)}{\mathrm{T}}=\frac{3}{n}$, saying that it did not matter which, because all that is required is a measure, and accord-

ing to that law, either T or $\dot{x}^2 + \dot{y}^2 + \dot{z}^2$, being always in the same ratio to one another, may be taken as the measure. But if we admit, as I think we must, that deviations from this law, small or great, must be always occurring, thanks to the ubiquitous ether or other disturbers of the peace, the question can no longer be evaded. On the whole, the tendency has been still to regard energy of translation as measuring temperature.

31. But to this Tait objects strenuously. I will endeavour to give his reasons. When (he says) there is molecular attraction, the mean kinetic energy increases with the density (which must be admitted on any theory), and so the "sorting demons," as Lord Kelvin calls them, might, by advancing from time to time those portions of an elastic boundary on which no impact is impending, diminish the volume, and so (if kinetic energy is temperature) increase the temperature, without doing any work. But why should they not? The increase of temperature is consistent with the conservation of energy, because it takes place at the expense of potential. And if it be inconsistent with any other law, *e.g.*, the second law of thermodynamics, is not that exactly the object for which Maxwell created the demons, *viz.*, to violate the second law without violating the conservation of energy? Is it not the law of their being? Violation of the second law is only *malum prohibitum.*

32. Tait again says, and I here quote his own words: "Let the contents of equal volumes at different parts of a tall column of gas be compared. In each the pressure may be regarded, so far as it is due to the external potential, as being applied by bounding walls. But the temperature is the same in each, and the only other quantity which is the same in each is E (*i.e.*, Rt). For as the particles are free to travel from point to point throughout the whole extent of the group, the average value of E must be the same for all, and therefore in regions where the density is small, it must be that of free particles, *i.e.*, absolute temperature."

I confess to having some difficulty in following this reasoning. I understand E or Rt to be the mean kinetic energy of a molecule during free path, and therefore in case

of a very rare medium the whole mean kinetic energy of a molecule, and therefore in that case the measure of temperature. In the case of a vertical column of gas I understand Professor Tait to assert that the constancy of temperature throughout (when the column is in equilibrium) is an axiom. Anyhow it is certain that in case of a rare gas the mean kinetic energy of a molecule is constant throughout the column, and if kinetic energy and temperature are the same thing no question arises. But when the gas becomes denser can it be accepted as an axiom that the temperature is constant?

Assuming it to be so, then if E is also constant throughout, we have sufficient grounds for taking E as the measure of temperature, that is, the mean kinetic energy during free path, instead of the whole kinetic energy. But the proposition that E is constant seems to me to require more proof than is given to it in the above extract. Then it may be asked what becomes of the temperature when the gas is so compressed that there remains no free path, which must surely be a possible condition above the critical temperature. Perhaps Tait would say that the substance must now be regarded as liquid. He goes on to say that for a liquid the temperature is not E but E + C, C being a quantity which is zero at the critical point, and increases with diminishing temperature.

Finally he says (part iv., p. 268): "What has been said above leads us in the succeeding developments to write (so long as we are dealing with vapour or gas) $E = Rt$ where R is now the increase of pressure with temperature under certain ordinary conditions. That is $E = t\frac{dp}{dt}$ or $\frac{dp}{dt} = \frac{E}{E+C}$."

We must now dismiss this subject in the hope that Tait will throw some more light on it in his forthcoming papers.

33. *On a dense medium of elastic spheres.*

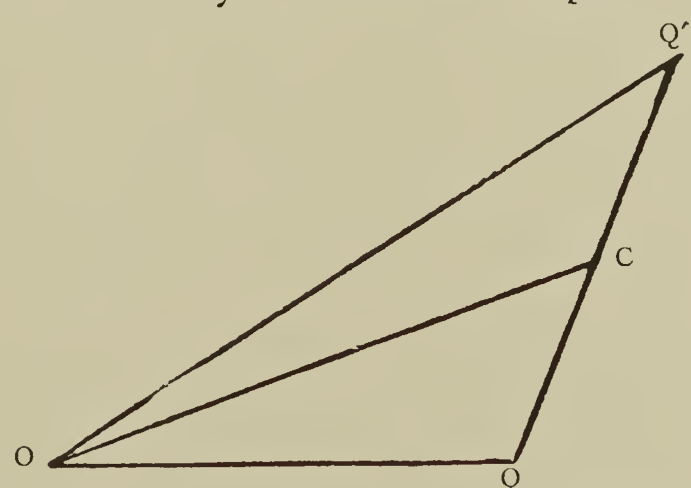
Tait's molecules are elastic spheres, although their motion is much affected by the assumed attractive force between them. It does not appear that any physicist has yet worked out the problem of the motion of a system of elastic spheres as such, when their aggregate volume comes to bear an appreciable proportion to the space in

which they move. Can it be taken for granted that the only effect of increasing density with which we need concern ourselves is the shortening of the mean free path? And if so what is the shortening of the mean path? Tait goes so far as to say that when the density reaches a certain point, an almost discontinuous diminution takes place in the mean free path. For which statement the reader may consult Tait (part iv., p. 265).

34. It appears to me that one effect of increasing density will be that the velocities of spheres which are near to one another will no longer be independent of one another, as in the ordinary rare medium they are assumed to be. Given that sphere A has positive velocity U in any direction, there will be a presumption, becoming greater as the density increases, that a neighbouring sphere B has some positive velocity in that same direction.

That statement can be proved as follows: Suppose a sphere whose velocity is ω in any direction, say that of x, to undergo a collision. What is the value of the expectation of its resolved velocity in x after collision? or (which is the same thing when the spheres are equal) that of the other colliding sphere after collision? Its accurate form can easily be found in the form of an integral for all values of the pre-collision velocity of the other sphere. For the present purpose I need only show that it is positive. Let V be the common velocity, ρ the half relative velocity of the two spheres. Let V = OC, and about C describe a sphere with radius ρ.

If ω = OQ, the pre-collision velocity of the other sphere is OQ′, where QCQ′ is a diameter of the ρ sphere. After collision all directions of the diameter QCQ′ are equally probable. We see that (1) if $V > \rho\sqrt{2}$, the cosine of the angle between the original direction of either sphere and its direction after collision is necessarily positive, (2) if $V < \rho\sqrt{2}$, the value of the expectation of it is positive,



but becomes zero in the limiting case when $\frac{\rho}{V}$ becomes infinite.

35. We can show secondly that the value of the expectation, though positive, is less than $\frac{\omega}{2}$. For if ω, ψ be the vector velocities of the two spheres before collision, their vector velocities after collision are in the notation of quaternions $\frac{\omega + \psi}{2} \pm \rho$, where ρ is a vector whose tensor is that of $\omega - \psi$, and for which all directions are equally probable.

Consequently the average direction for either is that of the vector $\frac{\omega + \psi}{2}$; and since $S\omega\psi$ is on the average negative, the resolved part in direction ω is on average less than $\frac{\omega}{2}$.

36. We might call the original velocity ω the *parent*, and the two after-collision velocities the *children*, and so in relation to the velocities acquired in subsequent collisions, ω is the *ancestor* and the others *descendants*. And we might say that the quality of the ancestor, *viz.*, positive velocity in x, survives to the descendants with intensity diminished at each generation, but this is true only on average, because at each generation is introduced a large element of chance in the vector ρ. If, therefore, sphere A have positive velocity ω in x, the chance that another sphere B shall have positive velocity in that direction is increased if it be given that (in the above notation) the velocities of A and B have a common ancestor. Just as the expectation that a man now at Cambridge may become a distinguished mathematician would be increased if we knew that he and Professor Tait had a common ancestor.

37. To apply this doctrine to our elastic spheres. Suppose a number of them to be contained within a small imaginary spherical surface S. If they be very closely packed, so to speak, they will for the most part not escape into the surrounding medium without first undergoing many collisions with one another, and so without their velocities, to continue my metaphorical language, becoming related to one another. And their velocities will be no longer independent.

Otherwise we may express it thus: If there be N spheres within S, the chance that their common centre of gravity shall have velocity $U \ldots U + dU$ in x is in the rare medium proportional to $\varepsilon^{-NhU^2}dU$, because in the rare medium the velocities are all independent. If now the surface S were an elastic boundary, the N spheres would by their collisions with it and with one another in a very short time acquire what Tait calls "mass motion" with velocity U in x, their relative velocities being those of a system of elastic spheres in the special state, see (5) above. But remove the elastic boundary and they will in the rare medium escape out of S, and mix with the surrounding spheres, in much less time than it takes them to subside into mass motion.

But in the dense medium the process of diffusion or escape out of S is slower, and the process of acquiring mass motion more rapid. So, as you increase the density, you increase the chance that for neighbouring spheres the velocities have a common ancestor or many such, and therefore the chance that they are coincident in direction. The spheres will develop a tendency to move together in masses, to form "streams" in fact.

38. In such a medium the chance that the N spheres within S shall have respectively x velocities $u_1 \ldots u_2 \ldots u_N$ is not, as it would be on the ordinary hypothesis, represented by $\varepsilon^{-h(u_1^2+u_2^2+\ldots+u^2_k)}$ but will be of the form

$$\varepsilon^{-a_1 u_1^2 \text{ etc.}\left(a_1u_1^2+b_{12}u_1u_2+a_2u^2_2+\text{ etc.,}\right)}$$

in which the co-efficient b_{rs} will vanish if the spheres to which u_r and u_s relate are not very near each other. And it will be found that the energy of the motion of the common centre of gravity of the group, *i.e.*, the energy of stream motion, bears a larger proportion to the whole energy of the group than it would do if the chances were all independent.

39. To work out this problem in a mathematical form would be very difficult. I can do no more within the limits of this article than indicate what, as appears to me, the general character of the motion will be. I hope that the long delay in the appearance of the concluding parts of Professor Tait's work is due in part at least to the fact

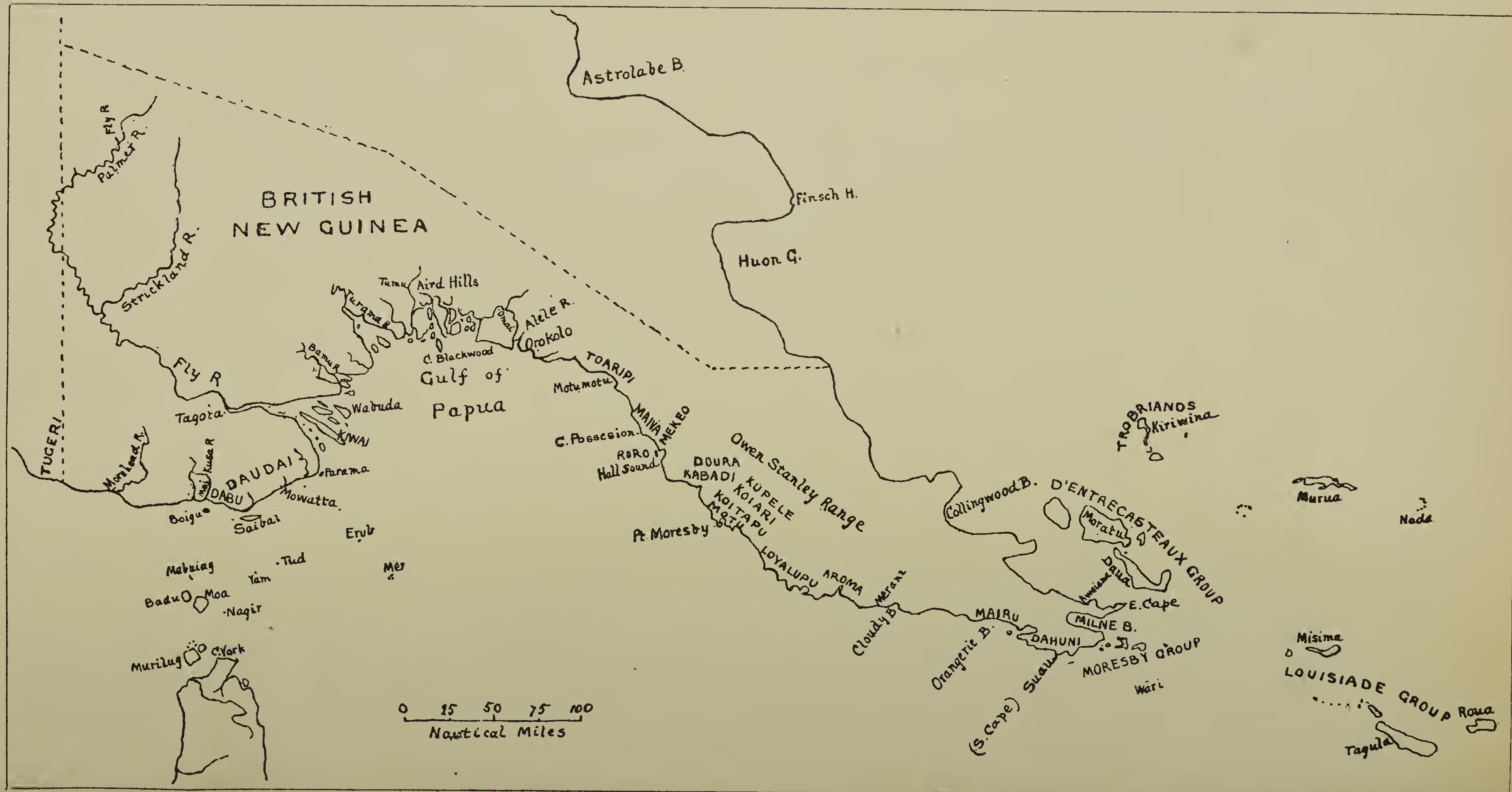
that he is working out a complete theory of a dense medium of elastic spheres.

40. We might perhaps carry the doctrine of "streams" farther. Such a stream as above supposed is always dissolving, the spheres comprising it escaping by diffusion into the surrounding medium, just as we are told the particles of water which form a visible cloud in the sky are not the same from instant to instant. And the state of equilibrium would be when streams form as fast as they are dissolved. But the energy of relative motion of the spheres forming a stream may be a little greater or less. The less it is, the less is the tendency to diffuse, and the longer the stream survives. So that in the case of equilibrium we should expect to find that those parts of the system which have stream motion in the highest degree would on the average have less energy of relative motion, and therefore greater density.

41. The following system would not indeed be in complete equilibrium, but might not require any great force to maintain it, namely, a number of dense masses moving in a comparatively rare medium, the spheres of the rare medium having considerably greater mean kinetic energy than that of relative motion of the spheres within one of the dense masses. So that if one of the dense masses were surrounded by an elastic envelope, the external pressure on that envelope due to spheres of the rare medium with higher mean velocities should be equal to the internal pressure due to the dense medium within. If there were an elastic envelope equilibrium would be complete. In the absence of an envelope the spheres of the dense masses would escape, but only by the comparatively slow process of diffusion at constant pressure.

42. Now I have assumed the molecules to be merely elastic spheres. If, as Professor Tait supposes, they also attract each other with finite force, that molecular attraction might overcome the tendency to diffuse, and the system described in the last paragraph might be permanent.

S. H. BURBURY.



BRITISH
NEW GUINEA
Astrolabe B.
Finsch H.
Huon G.
Fly R
Palmer R.
Strickland R.
Fly R
TUGERI
Morehead R.
Tagota
Kusa R
DAUDAI
DABU
Boigu
Saibai
Mowatta
Parama
KIWAI
Wabuda
Bamu R
Turama R
Tumu
Aird Hills
C. Blackwood
Gulf of
Papua
Alele R.
Orokolo
TOARIPI
Motumotu
MAIVA
MEKEO
C. Possesion
RORO
Hall Sound
DOURA
KABADI
KUPELE
KOIARI
KOITAPU
MOTU
Pt. Moresby
Owen Stanley Range
LOYALUPU
AROMA
Merani
Cloudy B.
MAIRU
Orangerie B.
DAHUNI
Suau
(S. Cape)
MILNE B.
E. Cape
Dawa
Moratu
D'ENTRECASTEAUX GROUP
Collingwood B.
TROBRIANDS
Kiriwina
Murua
Nada
MORESBY GROUP
Wari
Misima
LOUISIADE GROUP
Roua
Tagula
Erub
Mer
Tud
Mabuiag
Yam
Badu
Moa
Nagir
Muralug
C. York
0 15 50 75 100
Nautical Miles

THE ETHNOGRAPHY OF BRITISH NEW GUINEA.

II. A GUIDE TO THE LITERATURE.

A VALUABLE *Bibliography of New Guinea* was compiled by Mr. E. C. Rye in 1884 (*Roy. Geogr. Soc. Suppl. Papers*, i.), and this has been brought up to date by Herr J. D. E. Schmeltz in 1893; but the latter author has done more than this, he has added a number of distributional and bibliographical notes to the Memoir which he has collaborated with F. S. A. de Clercq, and has given tables showing the distribution of a large number of Papuan objects and customs. In the appended bibliography will be found the more important works of Dr. O. Finsch, to whom students of the anthropology of the Western Pacific are so greatly indebted; no one interested in Papuan ethnography can dispense with his *Samoafahrten*, the *Ethnological Atlas*, or the highly important series of papers in the *Annalen des k. k. naturhist. Hofmuseums* of Vienna, from 1888-1893. The *Ethnographical Album of the Pacific Islands*, drawn by J. Edge-Partington, is as invaluable to students of ethnology as it is to collectors and curators. The Rev. W. G. Lawes, of Port Moresby, has taken a large number of most excellent photographs illustrating Papuan ethnology, and he has generously deposited the negatives with Mr. H. King, George Street, Sydney, N. S. Wales, in order that anthropologists might have the opportunity of purchasing authentic photographs. Mr. J. W. Lindt, of Melbourne, too, has numerous beautiful photographs; but the best of these are now published in his *Picturesque New Guinea*. The present writer has in the press a memoir describing and illustrating the decorative art of the natives of British New Guinea, in which will also be found descriptions and classifications of implements, etc., and a compilation of many ethnographical data. Mr. J. P. Thomson has compiled a book

entitled *British New Guinea* from Sir William Macgregor's despatches; he gives only a *résumé* of these, omitting other people's labours. Mr. S. H. Ray has studied all the available material on the languages of the Possession.

The foregoing works deal more or less imperfectly with the whole or a considerable portion of the Protectorate. I have not alluded to publications such as articles in encyclopedias and the like, which are compilations from the less recent literature, and I have omitted numerous books and papers which are of little or no interest to anthropologists.

On analysing the literature it is evident that most of our valuable information is due to but a small number of naturalists and missionaries. Amongst the former I need only mention the names of Jukes, Macgillivray, D'Albertis, and Finsch, and of the latter Gill, Lawes, and Chalmers, have alone availed themselves of their unique opportunities, Macfarlane has put only a very few facts on record, the other British missionaries have done absolutely nothing for science. A few observations were made many years ago by the Marist missionaries on Woodlark Island, and recently the Jesuit missionaries on Yule Island and on the St. Joseph River have published some interesting facts.

Most of the expeditions undertaken for scientific purposes, or as newspaper enterprises, have added very few and in some cases no new ethnographical data. A few travellers—Moresby (1876), Comrie (1876), W. Y. Turner (1878), Stone (1880), Basil Thomson (1889), Forbes (1890)—have made small but valuable contributions to Papuan ethnography; but the bulk of those who have published papers or books have made extremely few original observations. Two authors, "Captain Lawson" and L. Trégance,[1] have invented "facts," their books being entirely false!

[1] *Adventures in New Guinea, the narrative of Louis Trégance, a French sailor, nine years in captivity among the Orangwŏks, a tribe in the interior of New Guinea.* Edited by Rev. H. Crocker: London, 1876.

Three traders only have directly given to science the benefit of their experience; these are E. G. Edelfeld (1887), E. Beardmore (1890), and W. Tetzlaff (1892); but A. Goldie has verbally given much information to travellers, who have not duly acknowledged the source of their facts, and Robert Bruce has been of great assistance to the present writer.

The Government officials connected with British New Guinea have done lamentably little for anthropology, with the exceptions of Hugh Romilly and Sir William Macgregor. The latter, judging from what I have seen of his writings and have read and heard about him, appears to be a model administrator, and the sciences of anthropology, botany, geography, geology, and zoology are greatly indebted to his energy and ability.

In the following guide to the literature on British New Guinea I begin at the west and finish at the coast boundary of Kaiser Wilhelms-Land. I have also added a few references to the German and Dutch possessions.

The present writer has brought together practically all that has been published, with additional information, on the two tribes inhabiting the islands of **Torres Straits**, and he will shortly bring out a monograph on these people which will be published by the Cambridge University Press. The craniological data are papers by Turner (1880) and Oldfield Thomas, and the measurements by Quatrefages and Hamy in *Crania Ethnica* and by Flower in the *Catalogue of the Royal College of Surgeons*.

There can be little doubt that the islanders must be regarded as essentially Papuans, though they are not typical; for example, according to some unpublished measurements, they have massive skulls, on the border-line between mesaticephalic and brachycephalic, whereas the measurements of fourteen Daudai skulls give a dolichocephalic index of seventy-one. These results do not agree with those of Oldfield Thomas in his excellent paper, but it is probable he was dealing with some Daudai as well as island skulls. Quatrefages and Hamy (pp. 207-210, 253-256) have drawn

attention to the mixture of races that occurs here. In appearance, intelligence, excitability, handicrafts and most of their customs, the islanders are also unmistakably Papuan.

The mainland between the Dutch boundary and the Fly River has been explored by several people, and is now being pacified by Sir Wm. Macgregor. In Beardmore's paper (1890), and in the notes which follow, will be found an account of the Daudai (**Mowatta**, etc.) people (*cf. C. A.*, 1, 1893, p. 35). Macgregor (*C. A.*, 1, 1892, p. 43) discovered the **Dabulai**, who inhabit the country opposite Saibai Island, and who are said to have affinities with the Western tribe of Torres Straits. In the same report he alludes, amongst other tribes, to the **Masingara**, who chew kava (*cf. J. A. I.*, xxi., 1891, p. 204; D'Albertis, ii., p. 197). The first accurate account of the Mai- and Wassi-Kussa Rivers is that by C. E. Strode Hall (*C.*, 5883, 1890, p. 213). Strachan's account apparently is very misleading. For accounts of **Tugeri** head-hunters, who come from Netherlands New Guinea to ravage the coasts and islands of the British Protectorate, *cf.* Haddon (1891); Macgregor (*C. A.*, 105, 1890, pp. 68-75; *J. A. I.*, xxi., 1891, p. 200; *C. A.*, 1, 1893, pp. 50-53). Little is known of the coast between Mowatta and the Fly River or of the adjacent islands (*cf. C.*, 6323, 1891, pp. 116-118; *The Arch. Rev.*, iii., 1889, p. 276; and Gill, 1876, pp. 219-242). Macfarlane in his "Notes from New Guinea," *Athenæum*, 1876, p. 725, expresses his view concerning the ethnology of these people.

All we know concerning the ethnography of the **Fly River** valley is due to D'Albertis and Macgregor. The latter says (*C.*, 6323, 1891, p. 113): "I have seen no evidence of the existence of a race in the interior distinct from the coast tribes. Those seen at the boundary between British and German New Guinea were of a light colour; but that may not signify much. The dialects of the lower tribes differ from those of the upper in, as far as we know, every word. Of the structure of the languages we know nothing. . . . They all use the bow and arrow. They suffer from the

same diseases. There is not a clay pot on the Fly River. . . . They have the same large breed of orange-coloured dingo. . . . They are equally shy and timid, yet vivacious, excitable, and always doing very plucky things." All the lower part of the river is swampy. **Kiwai** is the largest island in the delta, the natives cultivate thirty-six kinds of bananas, twenty kinds of yams, ten kinds of sweet potatoes; they use eleven kinds of fibres and drain their gardens by ditches four yards apart. The men are quite naked (*cf.* D'Albertis, ii., pp. 18-21, 43, 51; Macgregor, *C. A.*, 105, 1890, pp. 36-43). A fairly complete reprint of the latter is given in *J. A. I.*, xxi., 1891, p. 75, and a shorter one in Thomson (pp. 117-121). Bevan's sensational account (1890, p. 258) of the cannibal habits of the Kiwai natives is criticised by Macgregor (*C. A.*, 105, 1890, p. 38).

Macgregor gives (*C. A.*, 105, 1890, p. 45) an interesting description of the village of **Odagositia**, fifty-one miles up the Fly River. One house was about 520 feet in length and more than 30 feet wide inside, the interior "was a perfect model of cleanliness and order". This is apparently the village which was looted by Mr. Chester and D'Albertis on board the *Ellangowan* (D'Albertis, ii., pp. 38-41). Between **Tagota**, where Macgregor was unprovokedly attacked (*loc. cit.*, p. 46), and Everill Junction there are very few natives. About 380 miles up the river is the "**Villaggio dei Cocchi**," in which D'Albertis collected (!) a couple of pigs and a large number of "interesting objects" (ii., pp. 131-137), and consequently Macgregor received a hostile welcome (*C. A.*, 105, 1890, p. 52). The most remarkable objects D'Albertis brought away were stone-clubs with perforated ornamental tops, wonderfully carved out of hard stone; nothing like them is known from elsewhere. D'Albertis (ii., pp. 85-89, 95-103), and Macgregor (*loc. cit.*, pp. 54-61) describe the upper reaches of the Fly River and the peculiar large houses made by the people.

The country between the Fly and Aird Rivers has been visited and mapped by Macgregor (*C. A.*, 1, 1892, pp.

49-54; *C. A.*, I, 1893, pp. 37-43), thus completing almost the last part of the coast hitherto unexplored; not much is yet known about the natives. Bevan gives a little information about the natives of the river to which he gave the absurd name of Queen's Jubilee River.

The ethnography of the **Gulf Papuans** has been largely described by Chalmers in 1885, **Orokolo**: method of counting, three legends, the origin of mankind, the story of Kanitu and a deluge myth (p. 163), *dubus*, dance-masks and a *Semese* dance (pp. 233-235). Three visits to the Papuan Gulf are described, pp. 133-154, 192-204, 224-238. In 1887 Chalmers describes **Maiupa**, pp. 58-68 (also *cf.* Thomson, J. P., *Le Village des Cannibales de Maipua*, *Rev. d'Ethnogr.*, vii., 1888, p. 391); **Orokolo**, pp. 69-75, these people are small in stature, especially the women, and are very like the Koriari tribe at the back of Port Moresby; **Maclatchie Point**, pp. 82-90; **Motu-Motu**, pp. 280-284. This last place is now called **Toaripi** by Chalmers; the latter is apparently the native name, whereas the former is the name given to the village by the Motu. Chalmers also now speaks of the Toaripi tribe; formerly he called it the Elema tribe; the latter may be a more general term than the former.

A chapter on "**Motu-Motu** and the customs of the people" is contributed by E. G. Edelfeld to Lindt's book (1887, p. 132); he and Bevan (1890, pp. 138, 139, 144) allude to these people as tall, well-built, very noisy, egoistic and quarrelsome. Stalwart, hirsute men similar to these occur generally from about the Aird River to Cape Possession; but there are smaller, also dark people, within and beyond these limits. A short distance up the **Alele** (Wickham River) "the population is composed of black and light coloured natives, all cannibals" (Chalmers, 1885, p. 143). Bevan describes the natives on the **Aurarmar** or Arvei River (p. 239), and gives a figure of them (1888, p. 17), and in the latter book (p. 15) a photograph and description of the chief of Omai: "A little black man, with twinkling black eyes and pleasant features". Lyne (pp. 68-95) and Thomson (pp. 81-87) also refer to Motu-Motu. A very

valuable summary of the more important customs and beliefs of the Toaripi is given by Chalmers (1890, pp. 311-317) and a list of their games (1887, *A.*, p. 68).

That part of the British Protectorate which extends from Cape Possession (lat. 8° 30′ S.) to Orangerie Bay (about 10° 30′ S.) may conveniently be termed **The Central District**. It contains a large number of tribes, which may be roughly grouped into three classes: (1) The Papuan Hill tribes, (2) the Melanesian colonists, and (3) the mixed tribes. Speaking in general terms the Melanesian immigrants occupy the great part of the coast as far as Redscar Bay, having driven inland the autocthonous population, but everywhere there has been more or less a mixture of the two peoples. Even among some of the hill tribes there appear to be traces of racial mixture. From about Redscar Bay, or Hall Sound, to Cape Possession it seems that the coast population is essentially Papuan with an admixture of Melanesian blood. The main tribes of this district are the Maiva and Kivori, Mekeo, and Roro.

1. **The Mixed Tribes.**—The **Maiva** country, just south of Cape Possession, is the southern limit of dance-masks (Chalmers, 1887, p. 50). These people use the large oblong wooden shield characteristic of the Toaripi. Chalmers (1885, pp. 135, 185, 271-275; 1887, pp. 241-244, 270-274) describes the people, their sacred houses, or *dubus*, etc., and a Maiva sorcerer (pp. 310-318). Stone (p. 186) has a short account of the people, and valuable information is given by Edelfeld (p. 131). **Mekeo** is the name given to the populous and fertile district up the Paumumu or St. Joseph River; a very interesting description of it is given by Macgregor (*C. A.*, 1, 1890, pp. 76-83; *cf.* abstract in *Journ. Anth. Inst.*, xxi., 1891, pp. 201-204). An account of the trouble caused by some of the tribes and how Macgregor quieted them will be found in *C. A.*, 1, 1892, pp. 19-21; *C. A.*, 1, 1893, pp. 15-22). Edelfeld (pp. 126-130) was the first to describe this district. The people are industrious and intelligent; near Nauea is a regular market place, where three or four tribes meet on certain days.

Fowls are kept in coops. They excel in fine network and make hammocks. It is probable that the Maiva and Mekeo are closely related tribes. **Roro** is the native name for Yule Island, and it is employed for the cognate mainlanders. Moresby gives the first description of these people (p. 177); other accounts will be found in D'Albertis, i., pp. 243-421; Stone, pp. 18-30, 186, 205, 230; Lyne, pp. 48-67; Chalmers, 1885, pp. 166-191, 271; Letters from Verius, Couppé and Navarre in Jouët, pp. 130-311, the chapter by Bishop Navarre on *Manners and Customs of New Guinea* is especially interesting.

2. **The Hill Tribes.**—Macgillivray (i., pp. 293-298), Gill (1876, pp. 242-263), and Moresby (p. 139) describe the natives of Redscar Bay. The **Kabadi**, who live at the back of the Bay, have been graphically described by Chalmers (1885, pp. 155-160; 1887, pp. 98-110, 119-123); *cf.* also Macgregor, *C. A.*, 1, 1892, p. 25. They with their allies the Doura have a blood feud with Koitapu, and "have long been notorious as a bad lot" (Macgregor, *C.*, 6323, 1891, p. 10). They and the Nara seclude their girls for a year or two when about twelve years old (Chalmers, 1885, p. 159; 1890, p. 319). The **Doura** are a miserable, small tribe that has suffered much at the hands of its neighbours (Chalmers, 1887, pp. 91-98). Macgregor (*C.*, 6323, 1891, pp. 9-13) visited this country and ascended the Vanapa River. He describes the chief as a man "with a decided Semitic face," and goes on to say that the Oriental cast of features is characteristic of the tribes of the interior. He describes (*C. A.*, 13, 1890, p. 38) a remarkable rattan suspension bridge, which is figured by Thomson. **The natives of the Owen Stanley Range** have been described by Macgregor, *C. A.*, 13, 1890, p. 41; they speak a Papuan dialect, and are physically stronger than the coast men, with rather broad and prominent cheek bones, Semitic nose, strong underjaw, very voluble; they do not tattoo nor wear nose ornaments, they wear caps made of cuscus fur (*cf.* also Macgregor, *Proc. Roy. Geogr. Soc.*, xii., 1890, p. 193). H. O. Forbes, *P. R. G. S.*, 1890, p. 562) believes that these belonged to the same tribe he met with; he gives a good de-

scription of the people and their clothing, etc., and a method of making fire by sawing with rattan, Lawes (*Proc. Roy. Geogr. Soc.*, v., 1883, p. 357) records a similar process used by the Koiari; and D'Albertis also described an analogous method of fire making near the source of the Fly River (ii., p. 99). Chalmers (1885, p. 162) met some squalid natives from the Yule Range; the men sleep in hammocks and are said to trade in shell ornaments with the coast natives of the north-east coast of the Protectorate. The best account of the **Koitapu** (**Koita** of Macgregor) is that by Lawes (p. 371). They live at one end of the Motu villages, but preserve their distinctness, or in villages a little way inland. There is little difference in physique between these two tribes, but the former is a little the darker. Chalmers (1887, p. 13) refers to the relations between them; it is interesting to note that the Motu usurpers have to pay spiritual tribute to the Koitapu sorcerers and buy fine weather from them. A good account of Koitapu sorceries, sacred stones, charms for wind and rain, yam planting and fighting is given by him in 1887, *A.*, pp. 58-62. The two tribes never fight now, but the Motu often help the Koitapu against their enemies. He also alludes to them on pp. 110-115 (1887); and in 1885, p. 151, refers to a legend which points to common origin for the Toaripi, the Koiari and the Koitapu; in a later version (1887, *A.*, p. 57) the Motu also have a joint origin with these; Turner also refers to them (pp. 472, 487). Romilly (1893, p. 321) describes a Koitapuan fire hunt or kangaroo drive. Lawes (p. 374) says the last two tribes are closely allied; he refers to their varied physiognomy. The **Koiari** villages are built on crests of hills, and in almost every village is one house high up on a tree. Macgregor (*C. A.*, 1, 1892, p. 27) attributes the more sturdy build of the Koiari to their mountain life and abundance of food. Stone (pp. 113-125, 129, 164-168) also describes the appearance of the Koiari, their houses, clothing, etc. Quite recently a very valuable account of Koiari customs is given by Chalmers (1890, pp. 317-323). Inland of the Koiari lies the country of the **Kupele** or **Kubere**. Chalmers (1885, p. 130) says some are very dark, others very light

coloured; they are allied to the Koiari. The Tabure and Sogeri are similar to the Koiari.

3. **The Melanesian Colonists.**—Of these immigrants the **Motu** have been most written about. Moresby (pp. 154-158) was the first to discover Port Moresby, which is the centre of the Motu tribe and has since become the capital of British New Guinea. The port was next visited by Gill (1876, p. 263); W. Y. Turner gives an extremely good account of the people, which is supplemented by Lawes. Stone's account will be found on pp. 46-97, 193, 203. Chalmers (1887, *A.*, pp. 63-68) describes Motu marriage and funeral customs, their religion and other matters; the legend (p. 57) pointing to a common origin with the Koiari, Koitapu and Elema (Toaripi) by no means adequately accounts for the ethnological relationships of the Motu. Elsewhere (1887) he devotes chap. ii. to a graphic account of the preparations for the annual trading voyage and chap. viii. to a comparison of Motu and Toaripi customs and beliefs. The Motu make great trading voyages to the Gulf of Papua in October, *i.e.*, at the end of the south-east monsoon, and return during the north-west monsoon. They voyage in *lakatois*, each of which consists of at least three canoes joined together and rigged with sails like crabs' claws. The Motu women make pottery, and as many as 30,000 pots will be exchanged in one year, for 150 tons of sago. A fleet of twenty *lakatois* would carry 600 men, and each man takes about fifty pots (Romilly, 1893, p. 257). The voyagers in some cases go to a distance of 200 miles. Lindt gives excellent photographs of these strange craft and an account of them and the method of sailing them (pp. 29-47). Lawes (1888) has made a scholarly study of the Motu language.

In his map Chalmers groups the **Hula**, **Kerepunu** and allied tribes under the name **Loyalupu**. These people closely resemble the Motu, but appear to be somewhat lighter in colour. Stone (p. 190), Gill (1885, p. 288), Chalmers (1887, p. 323; 1887, *A.*, pp. 63, 65), Lyne (pp. 102-122) and Lindt (p. 67), allude to these people; the women are even better tattooed than the Motu. An annual ceremony

at **Kalo** is noted in the *Arch. Rev.*, iv., 1890, p. 149. The neighbouring **Aroma** tribe is referred to by Gill (pp. 297-301). Koapena, the chief of Aroma, has the reputation of being the finest savage throughout a large district; he has been described by Chalmers (1887, pp. 274-280), Lyne (p. 114), Finsch (1885), Romilly (1889, pp. 177-181) and others.

The **Mailu** or **Mairu** according to Chalmers occupy the coast from Cloudy Bay to Orangerie Bay. Thomson (pp. 42-47) conveniently summarises Macgregor's accounts of his visit to this district; especially interesting is the Administrator's account (*C. A.*, 105, 1890, p. 29) of the stockaded villages of Merani and Isimari; the former is provided with two tree-houses which serve the double purpose of watch-towers and for the defence of wall and of the western gate, the most vulnerable part of the village. The Cloudy Bay people have long had an evil repute (Romilly, 1889, p. 60, and others), but Macgregor has now brought them to reason. Lyne (pp. 135-143) describes a visit to Mairu (Toulon Island); Chalmers has apparently applied the name of the island to the people living on the coast.

The district around Tauwara (Milne Bay) and the various archipelagoes off the south-eastern end of New Guinea appear to constitute a natural region to which I have elsewhere (1894) extended the term Massim (*cf.* Hamy, *Rev. d'Ethnogr.*, vii., 1888, p. 503).

For an account of the **Suau** (South Cape) natives see Chalmers and Gill, 1885, pp. 40, 51, 329, 334, Lyne, p. 166; the latter describes a cannibal feast in **Milne Bay** (p. 200). Moresby, who first discovered this bay, found the natives "friendly from the first" (p. 216). Finsch gives an account of this district (1888, pp. 262-273), so does Bevan (p. 89). The hill-tribes are noticed by Chalmers (1889, pp. 140, 143). **Awaiama** (Chads Bay), on the north side of the peninsula, was the scene of the murder of a trader named Ancell. This story, with the retribution which followed, is told by Baden Powell (1892, p. 147) and officially by Macgregor (*C.*, 5883, 1890, pp. 275-277, 297-301; *C. A.*, 1,

1892, pp. 10, 63), also J. P. Thomson (p. 34). The population is divided politically into districts that form independent federated communities, none of which have a high chief, nor, as a rule, does any village acknowledge the undisputed sway of any one man.

The islands from South Cape to Teste Island may conveniently be termed **The Moresby Group**. They are inhabited by the same people as on the neighbouring mainland and there is a great deal of intercommunication. Macgillivray (i., p. 253) gives an interesting account of **Tassai** (Brummer Island), and Baden Powell (p. 157) describes **Samarai** (Dinner Island); Finsch alludes (p. 277) to finding tattooed people here and in Rogea, a custom which he had not seen in Milne Bay or in the neighbourhood of the D'Entrecasteaux. The charnel houses and other particulars of **Rogea** (Heath Island) and **Sariba** (Hayter Island) are described by Macgregor (*C.*, 5883, 1890, p. 305); on p. 302 he states the **Tubutubu** (Engineer Group) people are great traders; they procure their fine (fifty feet long) sea-going canoes from Murua. Moresby (pp. 182, 188, 202), Bevan (p. 87), and others describe these islanders as copper-coloured. Chauvin ("Mémoire sur les Races de l'Oceanie," *Arch. des Miss. Scientif*, 3[me] sér., viii., p. 452) says: "In the series [of sixteen skulls] from the Engineer Group we can form two groups, especially if we pay regard to the character of the face". Hamy quotes (p. 511) Chauvin and compares his results with those of Comrie (1877, p. 102, and *cf.* Flower's *Catalogue*); in referring to the lighter coloured individuals who have been noticed by all travellers, the latter says the features and hair even in the lighter individuals remain unaltered. The best account of **Wari** (Teste Island) is by Finsch (1888, pp. 271-286); he describes the natives, their tattooing, making and trading of pottery; the canoes are imported from Murua, but are decorated with carving here.

The Louisiade Group consist of several large and numerous small islands, which are really a continuation of the axis chain of New Guinea. Macgillivray (i., pp. 168-250) gives an account of the previous exploration of the

group and adds much interesting information. Macgregor (*C. A.*, I, 1892, p. 66) gives a very favourable picture of the inhabitants of **Pannaet** (Deboyne Island); they are active, intelligent, of good physique, and build the best canoes in the Possession; for this work they use adzes made of hoop iron, but sell the canoes for from ten to fifty stone axes. "They do not now use the stone axe as a tool in this part of the country, but it still represents the standard of currency in great transactions." The **Utian** (Brooker Island) are an aggressive and untrustworthy folk; they make clay pots. **Misima** (St. Aignan) is a large, mountainous and fertile island. The people are active, vivacious and industrious, but extremely untrustworthy. A large number of people associate together to make extensive gardens; having no sea-reef, there is no fishing. "They have entered the iron age and appear to have entirely given up the use of the stone axe except as a medium for purchasing wives" (Macgregor, *C.*, 5883, 1890, p. 249; *C. A.*, I, 1892, p. 69; *C. A.*, I, 1893, p. 32; and J. P. Thomson, p. 28). Basil Thomson (p. 533) noticed two types, "the one evidently Papuan, the other betraying strong Malay characteristics, such as the straight hair and not prominent features".

Tagula (Sudest) is the largest island of the group. J. P. Thomson (p. 15) refers to depredations wrought in this island by "the wild cannibal head-hunters of Brooker Island". Macgillivray (i., pp. 186-241) describes a couple of neighbouring islets. The furthest island is **Roua** (Rossel Island); it was here that the *St. Paul* was wrecked in 1858 with 327 Chinese emigrants on board, of whom all but one are said to have been massacred and eaten in about two months (*cf.* V. de Rochas, "Naufrage et Scènes d'Anthropophagie a l'Île Rossel," etc., *Tour de Monde*, 2me sem., 1862, p. 14). Macgregor (*C.*, 6323, 1891, p. 197) says the people "are perhaps the most harmless and inoffensive in the Possession. . . . They are not tattooed. . . . The women are very clever at basket making. . . . The men carry sponges to wash their faces with." Elsewhere (*C.*, 5883, 1890, p. 245) he says: "They have no pottery.

. . . The natives differ from those of the mainland, resembling in appearance a mixture of the Motu native and of the New Hebrides native." Basil Thomson (1889, p. 532) says: "Their appearance suggests a hybrid between the Papuan and the natives of the Solomon Islands. . . . They are short and robust, of sooty-brown complexion, with a flat nose, wide nostrils and markedly prognathous."

The D'Entrecasteaux Group consists of Duau (Normanby Island), Moratau (Ferguson Island), Goodenough Island and a few small islands. The large islands are mountainous, very steep and fertile; as there are no fringing reefs very little fishing is done, so the people are driven to agriculture, at which they are very expert, making terraces on the mountain sides. All the natives appear to be head-hunters. Moresby (p. 245) found them very friendly. "The Papuan type is well seen in" the natives of **Duau.** "There can be no doubt that the most advanced of them, those of the east end, are head-hunters. The inhabitants of the southern part of the island are very ready to trade. . . . Those of the central portion have no idea whatever of trade, and those of the north end have a less productive country and do not seem to possess the energy of those in the south" (Macgregor, *C. A.*, 13, 1890, p. 11; also *C. A.*, 1, 1892, p. 63; *C. A.*, 1, 1893, p. 23). Basil Thomson (p. 536) came across two tribes in the north not ten miles apart, one of which would barter all they possessed for tobacco, while the other did not know its use. The natives of **Moratau**, the largest of the group, are keen traders and have large canoes (Macgregor, *C. A*, 1, 1892, p. 65; 1893, p. 2; B. H. Thomson, p. 536). The Goodenough islanders are a friendly, undemonstrative people who make clay pots. "They are practically an inland or 'bush tribe'" (B. H. Thomson, p. 540). Sergi describes a large number of skulls from Dawson Strait.

The principal island of **The Trobriand Group** is **Kiriwina.** Only one language is spoken in the group; social matters are on quite a different footing from elsewhere in the Protectorate. It is very seldom that a woman or a boy

approaches a chief except in a crouching attitude; the chief is listened to and treated with respect. On the whole the women have less influence and have much less to say than is the case in many of the ruder tribes on the mainland. This is apparently a consequence of the superior position of the chief. They are a very hospitable people and presented Sir William Macgregor with quantities of cooked food in large wooden dishes; "nothing like this occurs on the mainland". They are skilful wood-carvers and keen traders. Their only weapons are spears, and short, double-edged clubs (*C. A.*, 1, 1892, p. 7; 1893, pp. 3-7, 28-30, and *J. A. I.*, xxi., 1892, p. 481; also Finsch, 1888, p. 205; Hamy; and Haddon, "Wood-carving in the Trobriands," *Illust. Archæologist*, i., 1893, p. 107).

Murua is by far the largest island of the small **Woodlark Group**. The first accounts of this island were given by Fathers Montrouzier (*Ann. p. l. Propog. d. l. Foi*, xxii., 1850, p. 88) and Thomassin (*l. c.*, xxv., 1853, p. 390). I have translated their most important observations in *Folk-lore*, 1894. Salerio refers to the people; he describes them as short, powerful; their skin colour is from pale yellow to chestnut brown; they are more friendly and less savage than the natives of the Louisiades (*Petermann's Mittheil.*, 1862, p. 343, with a good map on pl. xii.). Romilly (1886, p. 127) describes a visit to the island; he was struck with the good looks of the women; some of the men were dwarfish, they were of a light copper-colour. Macgregor (*C. A.*, 1, 1892, p. 4) formed a favourable opinion of the people—they are active and enterprising. The stone adze has quite disappeared; other details are also given. Sergi describes a number of skulls from Murua.

The natives of the **Laughlan Islands**, **Nada**, "are precisely the same (as those of the Woodlarks), and the same as the Papuans of the east coast of New Guinea. . . . They are extraordinarily friendly and never fight" (Romilly, 1886, p. 130). For a valuable account of native customs and traditions by W. Tetzlaff, see *C. A.*, 1, 1892, p. 104 (reprinted in *J. A. I.*, xxi., 1892, p. 483), *cf.* also Macgregor, *loc. cit.*, p. 9, and *Proc. Roy. Geogr. Soc.*, xiii., 1891, p. 177.

There is at present very little information respecting the natives of **the north-eastern coast** of the Possession. Captain Moresby was the first to explore this coast, and he gives a few notes on the natives. Macgregor (*C. A.*, I, 1892, pp. 11, 63) gives more details near **Cape Sebiribiri** (Cape Vogel); the natives are beginning to learn the use of tobacco and iron; they cook in profusely ornamented wide-mouthed clay pots. In **Collingwood Bay** "they did not understand the use of iron and had no wish to obtain any, of tobacco they were, of course, quite ignorant, and we found that the pieces of bamboo they carried were used exclusively as nose flutes". Moresby describes these people as dark and dirty-looking. Along this coast, according to Macgregor, the natives use disc-shaped stone clubs, spears and shields; they chew the betel, "but possess none of the dexterity in carving limespoons, etc., so conspicuous in the Trobriand Group. . . . They do not tattoo." Many wear the hair in long matted ringlets, some of the men wear false whiskers; they make elaborate corsets of net and inwoven Job's tears.

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KAISER WILHELMS-LAND.

The following works on German New Guinea should be consulted; a very complete bibliography is given by De Clercq and Schmeltz. Dr. O. Finsch's *Samoafahrten* may be regarded as the indispensable text-book of that district; his *Atlas* also illustrates many objects from the German coast, see also pp. 37-130 *Ann. k. k. nat. Hofmuseums. Wien*, iii., 1888, and Partington's *Album.* Dr. O. Schellong has made a number of valuable investigations among the natives; in his paper, Ueber die Herstellung einiger Ethnographica der Gegend Finschhafens, *Int. Arch. f. Eth.*, i., 1888, p. 220, he gives an illustrated account of the manufacture of beads from small shells, of bark cloth, and of small tortoise-shell earrings. There is a paper on Musik und Tanz der Papuas in *Globus* (1889), p. 81, and a very important study of the ceremonies of the circumcision of the lads, which constitute their initiation into manhood, Das Barlum-fest der Gegend Finschhafens (Kaiser Wilhelms-Land): ein Beitrag zur Kenntniss der Beschneidung der Melanesiër (*Int. Arch. f. Eth.*, ii., 1889, p. 145). He published in the same year another valuable paper on family life and customs, Ueber Familienleben und Gebräuche der Papuas der Umgebung von Finschhafen (K. W.-L.), *Zeits. f. Ethnol.*, xxi., 1889, p. 10. Many of the observations made by Schellong are also interesting from a medical point of view, as there is a good deal of physiological detail in this paper. In vol. xxiii., 1891, p. 156, of the same journal is a paper by Schellong entitled Beit-

räge zur Anthropologie der Papuas ; *cf.* also Die Eingeborenenbevölkerung (Papua's) von Kaiser Wilhelms-Land und dem Bismarck Archipel, *Beit. zur Allgem. Ztg.* (München), Nos. 46 and 48, 1889. Varied information and some papers of interest will be found in the *Nachrichten über Kaiser Wilhelms-Land und dem Bismarck Archipel* from the commencement in 1885. The following is the full title of H. Zöller's book, *Deutsch-Neuguinea und meine Ersteigung des Finisterre-Gebirges. Eine Schilderung des ersten erfolgreichen Vordringens zu den Hochgebirgen Inner Neuguineas, der Natur des Landes, der Sitten der Eingeborenen und des gegenwärtigen Standes du deutschen Kolonisation-sthätigkeit in Kaiser-Wilhelms-Land, Bismarck- und Salomon-Archipel, nebst einem Wortverzeichniss von 46 Papua-Sprachen*, 1891. Baron N. von Miklucho-Maclay, who lived among the natives of the Malay coast (Astrolabe Bay and its neighbourhood) for a long time, has unfortunately published very little ; the following notes of his embody some of his observations : Schädel und Nasen der Eingeborenen Neu-Guineas, *Zeits. f. Ethnol.*, v., 1873, verhandl., p. 188 ; Die Brachycephalie der Papuas in Neu-Guinea, *Zeits. f. Ethnol.*, vi., 1874, p. 177 ; Einige Worte über die sogennante "gelbe Rasse" im Süd—Osten Neu-Guineas, *Zeits. f. Ethnol.*, xii., 1880, pp. 90, 375 ; *cf.* also *Zeits. f. Ethnol.*, x., 1878, pp. 111, 116 ; xiv., 1882, p. 576 ; *Nature*, xxi., p. 227.

NETHERLANDS NEW GUINEA.

The most important work on Dutch New Guinea is a recently published memoir by F. S. A. de Clercq and J. D. E. Schmeltz entitled *Ethnographische Beschrijving van de West-en Noordkust van Nederlandsch Nieuw-Guinea*, Leiden, P. W. M. Trap, 1893. As this work must be consulted by any student of that portion of New Guinea, and as it contains such a complete bibliography, there is no need for me to copy out the titles of the works therein recorded. Most of the information respecting New Guinea in works on general anthropology and ethnology refers more particularly to the northern part of that island, since explorers like Wallace, Beccari, D'Albertis and Hartog, and vessels such as the *Challenger* and the *Marchesa*, have approached New Guinea from the Malay Archipelago. Prince Roland Buonaparte has several times epitomised the discoveries of Dutch travellers (*Bull. Soc. Géogr. Paris*, 1884, p. 530 ; *Soc. de Géogr. de Paris, C. R.*, 1885, p. 165, and separate publications in 1882 and 1886). The series of papers by A. B. Meyer, Ueber Hun-

dert fünf und dreissig Papua-schädel von Neu Guinea und der Insel Mysore (Geelvinksbai), *Mitth. ad K. Zoolog. Mus. Dresden*, i., 1875 ; ii., 1877 ; iii., 1878, and the other papers in that publication are important contributions to Papuan physical anthropology, as is Gabelentz and Meyer's Beiträge zur Kenntniss der Melanesischen Mikronesischen and Papuanischen Sprachen, *Abhandl. d. Phil. Hist. Cl. d. König. Sach. Gesel.*, viii., 1882, p. 375, from a linguistic point of view. It should be remembered that Schmeltz's bibliography is supplementary to that of Rye.

ALFRED C. HADDON.

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ON THE ARTIFICIAL HATCHING OF MARINE FOOD-FISHES.

THE artificial hatching of freshwater fishes, which was known to the Egyptians and Romans as well as to the Chinese and Japanese, has long been successfully practised, especially since Jacobi of Hohenhausen, in Germany (1748), and Remy, in France (1842), showed that it could be made profitable. Before the latter date, however, *viz.*, in 1837, John Shaw, of Drumlanrig, in our own country, had made important observations in connection with the eggs and young of the salmon. By-and-by the French, stimulated by M. Coste, who had the countenance of the Emperor, made remarkable strides in this department, so that the fine establishment at Hüningue, in Alsace, was soon equipped. The report of the French operations—especially that sent to Perthshire by Lord Gray—attracted the attention of those who took an interest in fish-culture, and various efforts—for example, those of Mr. Ashworth at the fishings of Galway—were made in this direction. The most notable, however, was the establishment, by the proprietors, of the fishings of artificial salmon-hatching at Stormontfield, on the Tay, in 1853. This well-known station proved of great service in giving opportunities for obtaining an accurate knowledge of the development of the salmon and its life-history, as well as in

promoting the prosperity of the salmon-fishings.[1] Since that date the hatching of freshwater fishes has been extensively carried out in the United States, Canada, France, Germany, Austria, Russia, Italy, Switzerland, Greece, China, Japan, Australia, New Zealand, and elsewhere, while in our own country it has also been largely developed by private enterprise, such establishments as those of Sir J. G. Maitland, at Howieton, being both extensive and successful. The British Government, however, unlike those of the United States and Canada, has not hitherto interfered with the subject, but has left it entirely in private hands. By means of this artificial method, streams, lakes, and ponds, in which the fish-supplies had either been reduced or removed, were re-stocked, and new forms introduced from distant waters.

While, therefore, the artificial hatching of freshwater fishes has thus been widely and successfully practised, it is otherwise with those inhabiting the sea. It is necessary, however, here briefly to review the situation in our country, in order to grasp the bearings of this important question.

Previous to 1883, our knowledge of the life-histories of the marine food-fishes was meagre. Few, or none, of our scientific men had studied their oviposition and development, so that such information had to be obtained by special inquiry for the Royal Commission then sitting (1883) under Lord Dalhousie. Fostered by the able and sympathetic chairman, and the other members of that commission, scientific knowledge of the fisheries generally, and of the development and life-histories of the marine food-fishes in particular, has made great progress; so that at the recent Committee on Fisheries of the House of Commons, under Lord Tweedmouth (an energetic member of the commission of 1883-84), the conditions were very different. This accurate knowledge of the subject has been obtained at the St. Andrews Marine Laboratory (under the Fishery Board for Scotland), the oldest in Britain; at the Granton Labora-

[1] Those familiar with this interesting spot will almost regret the transference of the main-hatching operations to Dupplin. The fry are now placed in the river at an early stage, and storage-ponds are unnecessary.

tory for a short time ; at the fine establishment of the Marine Biological Association at Plymouth, by the scientific members of the staff of the Fishery Board for Scotland, and by the recent expeditions of the Inspectors of Fisheries, Ireland, and the Royal Irish Academy. These united labours have not only made us acquainted with the spawning and development, as well as the whole life-histories of many of our most important marine food-fishes, have given us more extensive and accurate experience of the fisheries generally, but have stimulated practical energy in various directions, *e.g.*, in regard to shell-fishes and edible crustaceans. Much yet remains to be done, but those who had experience of the condition of our knowledge in 1883 will not be slow to accord due weight to the advances just alluded to, and especially to the labours of E. E. Prince,[1] T. Wemyss Fulton,[2] J. T. Cunningham,[3] and E. W. L. Holt.[4]

Such studies were absolutely necessary before proceeding, for instance, to hatch marine food-fishes on an extensive scale, and thus test the feasibility of increasing the numbers of the more valuable kinds by artificial methods. Moreover, they are equally important for purposes of legislation, in connection with the rate of growth and the attainment of maturity in fishes, the interdependence of the inshore and offshore waters—for example, the wafting of the eggs and larvæ of certain fishes shorewards, and the return of the young or adolescent fishes to the offshore waters ; while the food and the enemies of fishes at their various stages, the currents, temperatures, and the general physics of the water are also in need of further elucidation. These and many other problems are so closely interwoven with more purely scientific marine work that dissociation is impossible. Every nation with marine fisheries, indeed, is

[1] Now Commissioner of Fisheries in Canada.

[2] Superintendent of Scientific Investigations, Fishery Board for Scotland.

[3] Of the Marine Laboratory, Plymouth.

[4] Formerly of the St. Andrews Marine Laboratory, now at the Laboratory, Grimsby.

taking active steps to foster scientific inquiry in this direction, and contemplates, sooner or later, to carry out experiments in the hatching of marine food-fishes. Thus, for example, a year or two ago the German Government established at Heligoland an admirably equipped biological station for the scientific investigation of the North Sea fisheries.

It has yet, of course, to be proved that the artificial hatching of marine fishes, even on a large scale, will be beneficial to the fisheries generally; yet the importance of the issue demands an exhaustive trial. Almost everywhere during the last decade or two, complaints have been made as to the decrease of important marine food-fishes. Especially have the large halibut, turbot, brill, soles and other flat-fishes become rare. In Britain, this alleged diminution has been connected—for the last decade at least—with the extension of beam-trawling in our waters. Consequently, the legislature has closed the inshore area all round Scottish shores, and even considerably beyond that limit in certain places, as in the Moray Frith and the Frith of Clyde. Many thousand square miles of water are thus placed solely at the disposal of the liner.

The hatching of sea fishes on a large scale offers considerably greater difficulties than that of freshwater forms. The experimenter in his arrangements has occasionally to deal with the whole force of the waves in securing pure water for his tanks and ponds. Pumping is generally required, and the pipes and other apparatus must be specially constructed. Again, the necessary food for the minute and delicate young must be present in the tanks or other enclosures if he wishes to rear them beyond the larval condition; while predatory forms and decaying sea-weeds must be absent. Instead of young fishes, as in the salmon, which are capable of being handled with safety, the marine food-fish, with few exceptions, on its escape from the egg is a tiny, transparent creature, scarcely visible in the sea-water, and devoid of a mouth.

While here and there in our own and other countries

certain marine fishes had been hatched for scientific observations, yet to the Americans, under the late Professor Spencer Baird, belongs the credit of having first bestirred themselves in regard to the artificial hatching of marine food-fishes on a large scale. Since the establishment of the Fish Commission in 1871, various places on the coast have been selected for marine work of this kind, and finally the stations of Wood's Holl and Gloucester, on the shores of Massachusetts, were fixed on for permanent occupation and the erection of hatcheries, with, in the case of the former, a well-equipped laboratory. The Americans, indeed, with a breadth of view not always met with in our country, have ever held that it is not merely by the mechanical addition of fishes to our waters that the marine fisheries generally will be advanced. In other words, that it is on scientific investigation into marine life in general—whether directly or indirectly bearing on fishes and the fisheries, in addition to what may be termed fish-cultural work—that real progress depends. As Lord Playfair clearly put it in 1884: "Though the promise of practical utility from such (marine) laboratories is very great, that is not the first or the only thing to be considered. Laboratories of this nature, in which the habits of all kinds of marine life should be studied, ought primarily to be established, not with a view to practical uses, but with the main purpose of advancing science for its own sake. Science so studied rewards a nation a thousand-fold in the most unexpected practical applications; but without science there are no applications."[1]

As might have been expected, the hatching apparatus for the eggs of marine fishes was at first somewhat imperfect, and various methods—such as floating-boxes, plunging-buckets, Chester tidal-jars, and the universal hatching-jar—have since been adopted with more or less success. From the fact that most of the food-fishes have floating or pelagic eggs, which often rise near the surface of the water, quite a different arrangement is necessary from that, for instance,

[1] *Report of Meeting for Biological Investigation of Coasts of United Kingdom*, 31st March, 1884, p. 10.

required in the case of the herring with its adhesive eggs. The great principle is to keep the eggs in gentle motion amongst pure sea-water of the proper temperature. As first prominently brought forward by Lord Playfair, of St. Andrews, the Americans have chiefly experimented with the cod, and even so early as 1884 the fishermen on the neighbouring shores of the United States recognised the Commissioner's labours in shoals of young fishes which they termed "Commission cod". To a small extent, however, they have also dealt with the eggs of pollack, haddock, sea-bass (*Serranus*), Spanish mackerel, common mackerel, scup (*Stenostomus*), squeteague (*Cynoscion*), tautog (*Tautoga*), cunner (*Ctenolabrus*), with the sand-dab and the spotted flounder, both of which, like all the preceding, have buoyant or pelagic eggs, and with a flounder (*Pseudopleuronectes Americanus*), which has demersal (*i.e.*, non-floating) adhesive eggs. The anadromous shad has also been hatched in enormous numbers, and with very great success, but this form does not so much concern us at present. Altogether (up to 1891), larval cod to the number of eighty millions have been placed in the sea by the Commission; and Colonel Marshall Macdonald, the distinguished head of the department, is of opinion that as the result of these operations cod have appeared in places where they were formerly unknown. He specially points out that a variety with dark spots on the back, and hatched at Gloucester marine station, had been got for the first time in the waters near Plymouth Harbour in schools of fishes of about four pounds weight.

In the same continent, another station for the hatching of the ova of the cod has been for more than three years in operation at Dildo Island, Newfoundland, under the government of Canada. Up to 1893 (and for the three years) two hundred and twenty-two millions of larval cod had been placed in the water in an increasing ratio year by year. Thus in 1893, two hundred and sixty-five millions of the ova of the cod were placed in the hatching-vessels, and one hundred and sixty-five millions of larval fishes deposited in the sea, or about $62\frac{1}{4}$ per cent. At this station

the success in hatching the eggs of the lobster, a form rapidly diminishing in numbers both in Canada and the United Kingdom, has been noteworthy. Thus in the floating-boxes or incubators in 1893, four hundred and eighty-four millions two hundred and eighty-six thousand eggs of the lobster gave four hundred and twenty-seven millions two hundred and eighty-five thousand young lobsters, or 88·2 per cent., while in the hatchery proper another series of two millions five hundred thousand larval lobsters were produced. These valuable crustaceans are therefore readily hatched, but the difficulties begin shortly afterwards, since they devour each other so ravenously that the mortality is serious. Professor Prince, the Canadian Commissioner of Fisheries, indeed informs me that it is found necessary, at an early stage, to convey them in casks of sea-water to suitable sites in the inshore grounds, and cautiously sink them. As the casks have a large hole on one side, the young lobsters can freely escape at leisure into the ocean to follow their temporary pelagic existence. Professor Prince is of opinion that the Wilmot jar is, perhaps, the most successful apparatus for the hatching of these eggs. In Britain, it would be a very difficult task, even at Barra, to procure a tenth of the foregoing number of eggs, which are supplied by the various lobster "canneries" in the neighbourhood. In regard to the effects of these experiments on the surrounding waters, the fishermen attribute the shoals of young cod to the operations of the hatchery; but no observation of note appears yet to have been made concerning the lobsters.

Coming now to the continent of Europe, it is found that Norway, long so famous for its great cod-fisheries, holds a prominent place in marine fish-hatching, mainly through the efforts of Captain Dannevig, who, since 1884, has had charge of the sea-fish hatchery at Flödevig, Arendal. Year by year this energetic official has improved his methods, so that since the hatchery was established no less than nine hundred millions of larval cod have been placed in the fjords of the neighbourhood. Dannevig has also successfully reared the young cod in his enclosed ponds

to the second year. Constant fishing had greatly thinned the small variety of cod frequenting the Norwegian fjords, and there should be less difficulty, therefore, in determining the result of these experiments. For a year or two it is stated that the increase of cod on the inshore grounds has been marked, and further reports on the condition of these waters will be anxiously watched. Besides hatching the cod on a large scale, Dannevig has experimented successfully with the eggs of the flounder and the lobster. The apparatus used by Dannevig is that mainly employed at Dunbar, and will subsequently be described. In addition to the extensive hatching-house, a large salt-water basin for rearing the young, and a pond for the spawning fishes, are attached to the Norwegian station.

Though this country is thus not the first to make experiments in hatching marine fishes on a large scale, it is ten years at least since both this and other means of increasing the more valuable food-fishes were brought forward. Last year one of the methods was carried out by bringing several hundred soles from the east coast of England and placing them in St. Andrews Bay, where only a very few occur. This year the Fishery Board for Scotland was also able to commence the artificial hatching of sea fishes at Dunbar, where for some time they have been making preparations by constructing a hatching-house, spawning-pond, filtering-boxes, and by enclosing a tidal-creek for retaining the spawning fishes. As a careful account of the arrangements, which were founded on those of Captain Dannevig at Arendal, is given in the Report of the Fishery Board by Dr. Fulton,[1] with whom rested the responsibility of carrying out the whole system, only such structural features as are necessary to explain the method will be alluded to.

In selecting the species to be experimented with at Dunbar, it was thought desirable to begin with forms not only valuable in the market, but comparatively scarce in the neighbouring waters, and accordingly the turbot and sole were chosen. The former is, perhaps, the most valuable

[1] *Twelfth Annual Report* (1894), p. 196. Also Captain Dannevig's *Beskrivelse af Flodevigens Udklækningsauftalt ved Arendal*, 1892.

flat-fish in the British seas, and, moreover, it is tolerably hardy, so that it can be reared from young stages to the adult condition in tanks and confined areas of sea-water. The latter is also a valuable food-fish, and is hardy at all stages, while its comparative rarity in the neighbouring waters will make any increase after such operations unmistakable. A considerable number of soles had been collected last autumn and kept in the enclosed tidal creek at Dunbar, but as their spawning-season is somewhat late, it was decided to occupy the early spring months by experimenting with the plaice.

Steps were taken to secure adult plaice from the offshore grounds early in the season by aid of the *Garland*, and others were obtained on board various trawlers, though many of these afterwards succumbed from injuries received in the trawl. When the period for transference to the spawning-pond arrived, it was found that of fifty-eight adult plaice which had been confined for some months in the tidal-creek and had thus become acclimatised to their new life, all survived, while there was considerable mortality amongst those recently received. The greatest number of plaice in the spawning-pond at a given time was three hundred and ninety, and the whole number experimented with three hundred and ninety-six—a comparatively smal total when contrasted with the large number of cod kept in confinement during the operations of the Americans. The average size of the males was $17\frac{3}{8}$ inches, and of the females, $20\frac{7}{8}$ inches. The fishes fed readily in the spawning-pond, and swam actively about.

Spawning commenced in earnest on the 9th March, on which day two hundred and twenty-five thousand ova were collected, and continued till the 8th May, the total number of ova obtained during this period being twenty-seven million three hundred and fifty thousand. These large eggs had been hatched at the St. Andrews laboratory every season since 1884, and the larval fishes were known to be hardy, so that a good return was expected. Accordingly it was found that the number of larval fishes placed in the sea was twenty-six million and sixty thousand, the

loss during the operations in hatching being thus extremely small, *viz.*, only 4·4 per cent., a success largely due to the unceasing care of Dr. Fulton, Superintendent of scientific investigations of the Board, and to the enthusiasm and practical skill of Mr. Harald Dannevig, who had charge of the establishment. This result contrasts favourably with any hitherto obtained, especially when it is remembered that everything was new at Dunbar. If we, for instance, examine the American experiments at Wood's Holl during the season 1889-90, it is found that three thousand four hundred and three adult cod had been collected and retained in tidal-basins and live-cars for spawning purposes. Many of these, however, died or became blind, leaving at the end of the spawning-season only one thousand adults. From this large number of fishes, only eight million five hundred and forty-five thousand seven hundred ova were obtained, the mortality, further, during the hatching process being about 30 per cent., so that five million eight hundred and sixty-one thousand one hundred larvæ (fry) were liberated in the adjoining sea. Even a larger loss occurred in the case of one million one hundred and thirty-eight thousand two hundred ova of the haddock, for only five hundred and twenty-eight thousand larvæ were produced. A better result, however, took place with five million eight hundred and forty-one thousand one hundred eggs of a flat-fish (probably *Pseudopleuronectes*), since four million and eighty-six thousand seven hundred larval fishes were obtained for liberation. At Dildo hatchery in Newfoundland, the eggs of the cod have also been dealt with, and apparently with greater success than at Wood's Holl, for in 1892, the third year of the experiments, the total number placed in the sea when about a week old was one hundred and sixty-five millions, or nine times more than the first season. The loss, even in the latter instance, is greater than in the case of the plaice at Dunbar.

Again, at Wood's Holl, three thousand adult cod yielded from November, 1890, to February, 1891, sixty-seven million three hundred and ninety-nine thousand ova, from which thirty-six millions of larval fishes were obtained,

the mortality being thus very serious. In the instance of forty-three millions of ova sent by rail from Gloucester station, only one hundred and fifty thousand larvæ were forthcoming for liberation. The same species of flounder as in the preceding season gave four million six hundred and eighty-nine thousand seven hundred ova between February and March, from which issued three million three hundred and fifty thousand eight hundred larval fishes.

At Gloucester marine station in 1889-90, forty millions of the ova of the pollack produced fourteen millions of larvæ. From forty-seven millions of the eggs of the cod fourteen millions of larvæ were obtained; while from thirty millions of the eggs of the haddock, only five millions of larvæ were liberated. No statistics are given for 1890-91, except that thirty millions of the eggs of the cod collected in the neighbourhood, and, therefore, favourably situated, gave 25·5 per cent. of larvæ.

At Dunbar, the ova are voluntarily shed in the spawning-pond, which is forty and a half feet long by eleven feet two inches deep, the breadth varying from twenty-six and a half feet at the one end to eighteen feet at the other, and into which clean sea-water is sent in a full stream from a three-inch pipe of galvanised iron. The current carries them to a trough called the spawn-collector, which at first had only a frame of horse-hair cloth, but the eggs are now more expeditiously collected by a fine gauze bag through which the water passes, and which only requires to be gently inverted into a suitable vessel of sea-water to secure the eggs in perfect condition. Before collecting the eggs for the day, a larger current is made to pass out of the pond, thus increasing the number of eggs in the collector. The ova are measured and counted, and then placed in the hatching-boxes, each of which has the same length and depth, *viz.*, eleven and a half inches, and ten and three-quarter inches broad, while the bottom is of hair-cloth.

The boxes are attached by leather hinges to the partitions, and when the troughs are full of water the free end rises until it projects about three inches above the water,

and here comes in one of the features of Dannevig's system. The water which carried the ova to the spawn-collector rushes down an incline to a water-wheel, which moves an eccentric wheel or cam in the hatching-house. This gives an up-and-down movement to a bar of iron which passes between each series of hatching-boxes (five in number on each side), and has five short transverse pieces, each of which rests on a box. When the rod is raised by the wheel, the buoyancy of the boxes causes them to rise as already mentioned; but twice every minute the rods fall by the action of a beam on the cam, and each hatching-box is pressed under water by the transverse piece resting on it. Thus, besides the lively current of sea-water constantly flowing in by the broad metal spout at the upper end of the box, the rush of water through the horse-hair cloth on the bottom causes the ova to be in constant movement, and very regularly distributed throughout the box. The sea-water for this purpose is passed through a series of flannel filters arranged on each side (*i.e.*, in duplicate) of the slope between the spawning-pond and the hatching-house, and thus perfect purity is secured. Nowhere could healthier eggs be seen, and when the characteristic yellow pigment appeared in the embryos within them they were even beautiful. At the beginning of the work and when the temperature of the water had an average of 5·24° C., Mr. Dannevig found they hatched in twenty-one days, while towards the end of the operations and when the average temperature was 8·86° C., they issued in fourteen days. He kept some in the vessels till the yolk was absorbed and the mouth open, and for a considerable time thereafter, in all for forty-seven days, when an unfortunate accident to the water swept them off. Shortly after hatching (a day or two), the larval plaice turn their heads against the stream and keep in this position, as so frequently seen in the case of the young salmon at Stormontfield. The larval fishes, as a rule, are kept in the same compartments until the yolk is nearly absorbed, that is, from ten to fourteen days. They are then conveyed by the steamer *Garland* to the areas selected in boxes, which are

gently lowered into the water, and the little fishes soon spread themselves around.

On the other hand, the Americans kept the fry of the cod only from twelve to forty-eight hours after hatching. Here it was thought prudent to liberate the larval plaice at distances varying from two to six miles from shore, so that the usual transformation might ensue before the latter was reached. Under present circumstances, no question could be raised as to the retention or dismissal of the larval fishes, since no pond was available. Moreover, it is an open question whether the retention of the young fishes—until they have attained some size—would be a marked advantage. The expense would certainly be greater. Nowhere can these minute fishes find such nourishment as unfailingly meets them in the sea, and nowhere will they grow more rapidly. It is true that, translucent though they be, their natural enemies will reduce their numbers, but such is unavoidable. For the elucidation of the life-histories, again, of some, *e.g.*, the haddock and lobster, and the rate of growth of others, a certain number can be experimented with in tidal-ponds, as soon as these are available.

There is, perhaps, no urgent need for taking the round fishes in hand at present, since cod and haddock are in fair abundance, but if it were possible to increase the numbers of halibut, turbot, and soles, the benefit to the fishing community and the public would be great. The difficulties connected with the artificial hatching of these on a large scale are somewhat greater than in the case of the round fishes, but it is to be hoped they will be overcome. If it be found that the sole will live and breed in fresh water, and that the turbot, the brill, the bass, and the grey mullet—as stated so far back as 1837—will live in a five-acre lake so fresh that cattle drank it for nine months of the year, encouragement of a welcome kind is given to these hatching operations. Again, by the acclimatising of certain fishes, *e.g.*, the Baltic herring, to fresh water, a more successful means of transmitting the species to distant seas may also be offered than by the attempt to forward the eggs.

Amongst the pelagic eggs, besides those above men-

tioned, specially suitable for hatching on a large scale, are those of the hake, ling, torsk, lemon-dab, cod, haddock, whiting, bib, green cod, pollack, brill, dab, long rough-dab, sail-fluke, and gurnard. The large eggs of the wolf-fish (cat-fish of the fisherman), which adhere together in masses, can also be hatched with ease, and the young are remarkably hardy. Again, the eggs removed from "berried" lobsters may be hatched in the foregoing boxes or in Wilmot jars, and the larvæ either kept in extensive ponds (where, however, their habits of cannibalism are troublesome), or, as in Canada, carried seawards and sunk in large barrels with a hole on one side; and since great numbers are required, the neighbourhood of good lobster-ground, as at Dunbar or Barra, would be an advantage.

The establishment at Dunbar just referred to is thus a small beginning in this important field. We have only stepped on the threshold of the subject. A liberal and wise expenditure is absolutely necessary to carry out the experiments on a scale sufficient to test their real influence on the fisheries. Moreover, not only are skill, enterprise, and perseverance indispensable on the part of the officials, but patience on the part of the public. Those most familiar with the department, while, perhaps, by no means sanguine, feel that we have now reached a stage when the subject calls to be dealt with in a comprehensive manner. It is the practice in certain quarters to sneer at the supposed small results science can show, especially in Scotland, for the expenditure in connection with the fisheries. The total sum, however, for scientific investigations is only £3000; and if the cost of the small steamer *Garland* required for the survey and protection of the great areas (thousands of square miles) now reserved only for the liners, be deducted, the comparatively small sum of £1800 a year remains. This income has to bear the salaries of the staff, experiments in fish-, lobster-, and mussel-culture, the cost of apparatus, the marine laboratory at St. Andrews, the hatchery for marine fishes with its small laboratory at Dunbar, and the carrying out of other scientific fishery work. In England, for instance, the official expenditure in

the Fisheries Department of the Board of Trade nearly reaches the first sum (£3000), and yet no scientific investigations, no expenditure for a steamer, and no marine fish-hatching have to be met. Indeed, another £1000 is given by the Government to the marine laboratory at Plymouth for scientific fisheries' work, making the annual income of this establishment about £2200. As Dr. Fulton has shown, the United States spends annually £70,000 on fish-culture and scientific investigations, and employs two large steamers and a sailing vessel exclusively for the work. Besides this large sum, the fish commissioners of the various States also disburse considerably on the development of their fisheries. Canada, again, expends £100,000 yearly on her fisheries, of which a sum of about £10,000 is devoted to fish-culture.[1]

Finally, were the establishment of a proper series of statistics the only benefit that had already accrued to the fisheries of the country since scientific attention had been directed to the subject, such would be no trifling boon. Overweening confidence on the one hand, and pessimistic views on the other, have been toned to moderation, and at least a basis for guidance afforded where only conjecture existed. But during the last decade the studies on the reproduction, development, and life-histories of our food-fishes and shell-fishes, and the distribution of the immature fishes, have made it possible to proceed with the artificial culture of them. Much knowledge has also been gained concerning the rate of growth, the size at which maturity occurs, the food—both on the bottom of the sea and in mid-water—the latter in abundance at all periods of the year, and often presenting no connection with locality. Besides, in questions more or less practical, such as the

[1] Keeping the £1800 for Scotch scientific fisheries' investigations in view, a glance at the estimate for sundry grants in aid of scientific investigations, etc., in the United Kingdom (year ending 31st March, 1895), and amounting to £26,247, may be taken. Of this sum, £21,800 goes to England, £2877 to Ireland, and £1570 to Scotland. But, in addition, provision is made in other estimates for expenditure in connection with such service of £17,834, the total thus being £44,081.

formation of a mackerel fishery, the capture of immature fishes, and the influence of beam-trawling on the fishing-grounds and the abundance of food-fishes, important results have been obtained. These and other investigations, moreover, are watched with the closest attention by the fishery authorities on the continent and America.

It is true much yet remains to be done ; thus, even the ripe eggs and larval stages of such ubiquitous forms as the eel and the conger are unknown. It is certain, however, that a store of knowledge is being gradually accumulated—knowledge not only of value now, but that will be permanently advantageous to the department.

W. C. McIntosh.

THE MOLECULAR WEIGHT OF LIQUIDS.

THE properties of matter may be conveniently divided into three main classes: i., Additive; ii., Constitutive; iii., Colligative.

In determining the molecular weight of a substance, no matter whether it be gaseous or liquid, it is necessary to make use of some colligative property of the matter under investigation. The present article will therefore deal almost exclusively with colligative properties, or rather with a particular colligative property of liquids. As regards the molecular weight or complexity of solid substances it is well to state, at the outset, that our stoichiometrical knowledge of the properties of solids is so limited that up to the present time we have absolutely no reliable information on this point.

If a substance plays a part in a chemical phenomenon in such a way that any particular property of the substance remains unaltered or independent of its state of chemical combination, then this property is said to be additive. The best known additive property of matter is its *mass*. Thus, the mass of a substance undergoes no alteration during a change in its state of chemical combination. This fact finds its expression in the well-known law of the " Conservation of Matter ". Properties, such as melting- and boiling-points, molecular volume, etc., which depend to a large extent on the arrangement of the atoms in the molecule, have been termed constitutive. It very often happens, however, as in the examples just quoted, that a substance exhibits properties which are partially additive and partially constitutive.

The third group of properties is characterised by the fact that for chemically comparable quantities of the most widely different substances, a colligative property has always the same value. Whilst the mass of a compound is the sum of the masses of the elements of which it is composed, the volume of a gas, under the same conditions of temperature and pressure, is a colligative property and is

quite independent both of the chemical nature of the gas and of the number of atoms in its molecule. If we take a certain volume of hydrogen and convert it into water gas, H_2O, the volume remains unaltered. Further if we suppose that by adding ethylene C_2H_4 we could convert the water into alcohol gas C_2H_6O, the volume would still remain unaltered. By adding ethylene once more we may suppose that butyl alcohol, $C_4H_{10}O$, is formed. The volume of butyl alcohol gas would be precisely that of the hydrogen from which we started, and so on.

Just as the presence of additive properties in matter forms the foundation of the Atomic Theory, so the appearance of colligative properties forms the basis of the Molecular Theory. Colligative properties may be explained on the assumption that there exist certain independent groups of atoms, the molecules, which behave entirely in virtue of their number and irrespective of their nature and chemical composition. These colligative properties therefore lead to the molecular theory, and every such property may be made use of in order to determine the relative complexity or number of the molecules.

For example, according to Avogadro's law, equal volumes of gases, under the same conditions of temperature and pressure, contain the same number of molecules. This law may also be stated in another way, namely :—

Under the same conditions of temperature and pressure the molecular weights of gases are directly proportional to their densities.

It is thus easy to determine the molecular weight of gases from their densities, the molecular weight of hydrogen being chosen = 2 for reasons which are well known to chemists.

If it were found necessary to determine the molecular weight of a gas without a knowledge of its density, this could be done by measuring the rate of alteration of its volume energy with temperature. The volume energy of a gas is the product of its volume and pressure, pv; and if we choose as the unit of volume of a gas, the volume in litres occupied by the molecular weight expressed in grams, and

as unit of pressure, an atmosphere, then the volume energy of a gas is equal to a constant multiplied by the absolute temperature, thus: $pv = RT$. The constant R, for all gases which do not dissociate, is 0·0819. If, then, in investigating an unknown gas, we happened to find that its volume energy increased proportionately to the absolute temperature, and that the proportionality factor was 0·0819, we should be justified in concluding that the molecular weight chosen in expressing v was the correct one. But, if the proportionality factor were $2 \times 0·0819 = 0·1638$ we should then conclude that the number chosen for the molecular weight, and consequently the volume of the gas, was twice as great as it should have been.

If the factor had a value intermediate between 0·0819 and 0·1638 the deduction would follow that we were dealing with a mixture, and, finally, if the value of the factor were found to vary with rise of temperature, we should conclude that the composition of the mixture was altering. The volume energy would then be no longer a rectilinear, but a curvilinear function of the temperature, and the composition of the mixture could be deduced at any given temperature by a comparison of the slope of the tangent to the curve at that temperature with the slope of the normal line, expressed by the number 0·0819.

This method offers no advantage over the ordinary method for determining the molecular weights of gases, but an account of it has been given here, as an analogous method has been employed to determine the molecular weight of liquid substances.

To the Hungarian physicist Eötvös belongs the credit of having first shown (1886) that what has since been termed the *molecular surface energy* by Ostwald, is, between certain limits, a linear function of the temperature. Eötvös also observed that there were certain exceptions to the rule, and he pointed out that these exceptions could be made to disappear by choosing appropriate molecular weights which were higher than those ordinarily accepted.

Reference has already been made to the gas equation $pv = RT$, where p is the pressure of the gas, v the volume contain-

ing equal numbers of molecules, R a constant, and T temperature reckoned upwards from absolute zero, where pressure disappears. Ramsay and Shields have pointed out that there exists an analogous equation for liquids. If we represent the surface tension of a liquid by γ, the surface on which equal numbers of molecules lie by s, a constant analogous to R by k, and temperature measured downwards from the critical temperature, where the surface tension becomes zero, by τ, then the analogous equation for liquids is, $\gamma s = k\tau$. To this equation, however, a slight correction in the shape of another constant, d, must be added, as the line representing the product of surface tension into molecular surface has its origin, not at the critical temperature, but usually about $d = 6^\circ$ below it. The equation, therefore, becomes $\gamma s = k(\tau - a)$.

As gases have never been investigated at temperatures closely approaching the absolute zero we have no means of knowing whether a correction, similar to d, should be applied or not.

All the terms in the above equation are either known or susceptible to measurement. Eötvös determined the surface tension by an optical method, Ramsay and Shields by measuring the capillary rise in a tube of narrow bore.

In order to obtain good results several refinements of the ordinary methods are necessary, for the details of which the original memoirs should be consulted. In a good many instances the surface tension has been determined for a range between the ordinary temperature and the critical temperature; in some cases even as low as – 90°C.

Assuming that the distribution of the molecules is the same on the surface of a liquid as in the interior, the next term in the equation, s, the surface on which equal numbers of molecules lie, is determined as follows. If v is the specific volume of the liquid and M its molecular weight, then Mv is its molecular volume, that is to say, the volume in cubic centimetres occupied by a gram-molecule of the liquid. Now, let us conceive a cube with a volume equal to the molecular volume of the liquid; the cube root of the molecular volume, on the above assumption, obviously gives

the relative number of molecules along a side of the cube, whilst the square of this number represents the relative number of molecules distributed on a face of the cube. For different liquids, therefore, the molecular volume raised to the power two-thirds $(\mathrm{M}v)^{\frac{2}{3}}$, represents the surface on which the *same* number of molecules lies. This surface s is sometimes called the molecular surface of the liquid, so that instead of s in the above equation we may substitute $(\mathrm{M}v)^{\frac{2}{3}}$. The critical temperature and the correction d being determined, we have all the requisite data for calculating the value of k by the equation $\gamma\,(\mathrm{M}v)^{\frac{2}{3}} = k\,(\tau - d)$.

After carefully investigating a series of liquids Ramsay and Shields found that the mean value of the constant k was 2·121. All liquids, however, as Eötvös also found, do not give constants approaching the value 2·121. The chief exceptions are water and the fatty acids and alcohols, which have values less than 2·121. It is noteworthy that in the case of the apparent exceptions, the values of k vary with the temperature and more nearly approach the normal value the higher the temperature. In other words the molecular surface energy, the product of the surface tension γ into the molecular surface $(\mathrm{M}v)^{\frac{2}{3}}$, is no longer a linear function of the temperature.

Returning once more to the equation $\gamma\,(\mathrm{M}v)^{\frac{2}{3}} = k\,(\tau - d)$, it follows that the quotient of the differences of the molecular surface energy between any two temperatures by the difference in temperature, should remain constant, that is—

$$\frac{\gamma(\mathrm{M}v)^{\frac{2}{3}} - \gamma'\,(\mathrm{M}v')^{\frac{2}{3}}}{\tau - \tau'} = k,$$

γ and v corresponding to the temperature τ, and γ' and v' to the temperature τ'.

In the case of a great many liquids this was found to be the case. The variation in the constant from 2·121 lies within the limits of experimental error in the case of such compounds as the esters, the hydrocarbons, carbon tetrachloride, chlorobenzene, ether, mercaptan, the mustard oils, chloral, paraldehyde, benzoic aldehyde, nitrobenzene, aniline, pyridine, quinoline, silicon tetrachloride, phosphorus trichloride, phosphorus oxychloride, sulphur

chloride, thionyl chloride, sulphuryl chloride, nickel tetracarbonyl, etc.

It has already been pointed out that, in the gas equation pv = RT, the value of R is 0·0819, and that it does not vary with the temperature provided the molecular weight of the gas has been correctly chosen and that no dissociation occurs. If, however, dissociation takes place the value of R increases with rise of temperature. As we shall see later on, there is a great deal of evidence to show that the molecules of certain liquids, in passing from the state of vapour to that of liquid, associate to form more complex molecules, and that these liquids are precisely those which possess an abnormal and variable value of k. The effect of association would be that the real molecular weight would be chosen too low, and consequently that the value of k would appear to be less than 2·121. Furthermore, if such an associated liquid dissociated on heating, the low abnormal value of k would gradually increase. This is, in fact, what actually takes place, and Eötvös pointed out that the degree of association or the molecular complexity of the liquid could be calculated on this basis. The following experimental results, for example, have been obtained by Ramsay and Shields :—

Methyl Alcohol.		Ethyl Alcohol.		Formic Acid.		Acetic Acid.	
Temp. Interval.	k.	Temp. Interval.	k.	Temp. Interval.	k.	Temp. Interval.	k.
16°- 46°	0·933	16°- 46°	1·083	17°-46°	0·902	16°- 46°	0·900
46°- 78°	0·969	46°- 78°	1·172	46°-80°	0·911	46°- 78°	0·953
78°-132°	1·046	78°-132°	1·352	—	—	78°-132°	1·074

It will be seen that the value of k differs widely from 2·121 and that it gradually increases with rise of temperature.

The question which now rises is how the degree of

association or molecular complexity at any particular temperature can be calculated. We have already seen that the behaviour of non-associating or "normal" liquids may be expressed by the equation—

$$\frac{\gamma' (Mv)^{\frac{2}{3}} - \gamma' (Mv')^{\frac{2}{3}}}{\tau - \tau'} = 2{\cdot}121,$$

whilst for associated liquids—

$$\frac{\gamma (Mv)^{\frac{2}{3}} - \gamma' (Mv')^{\frac{2}{3}}}{\tau - \tau'} = k,$$

where k is a number varying with the temperature, and less than 2·121.

At each particular temperature we can multiply the gaseous or normal molecular weight of the liquid by a factor x, which is chosen so that the k of the last equation becomes 2·121; that is to say, we can select a molecular weight for the liquid which gives the normal constant 2·121.

The factor x at each temperature represents the molecular complexity of the liquid at that temperature. At present, unfortunately, there is no way of verifying this method of calculating the molecular complexity of liquids. It is still open to adverse criticism on the score that no rigorous proof has been brought forward to show that the true molecular complexity is obtained in this way, but it must be admitted that we can get an approximation to the molecular complexity, and future investigations will show how close the approximation is.

In the case of the normal liquids the experimentally found values of the molecular surface energy can be reproduced by the equation $\gamma\ (Mv)^{\frac{2}{3}} = 2{\cdot}121\ (\tau - d)$ up to within about 30° from the critical point, and by a slight alteration it may be made to fit right up to the critical point.

By the addition of another constant μ it was found that a formula of the form—

$$\gamma\ (Mv)^{\frac{2}{3}} = \frac{K\ (\tau - d)}{1 + \mu\tau}$$

agrees admirably with the experimental values of the molecular surface energy of methyl and ethyl alcohols, water

and acetic acid, between low temperatures and about 30° from their critical points. In the case of methyl and ethyl alcohols the agreement extends even as low as – 90°C. Agreement near the critical temperature may also be secured by a slight alteration in the formula. Neglecting, for the present, the slight alteration, the constants for these associating substances are :—

	K	d	μ	Critical Temperature
Methyl alcohol	1·489	–4·22	0·00104	240·0
Ethyl alcohol	2·170	4·8	0·00193	243·1
Water	2·631	19·5	0·00218	358·1
Acetic acid	1·910	11·9	0·00163	321·5

Having found a formula which reproduces the experimentally found values of the molecular surface energy of associating liquids at different temperatures, we are in a position to calculate the degree of molecular complexity of the liquid in the following manner. Supposing that the liquid is composed partly of complex molecules and that x is the measure of complexity, we have, as before—

$$\gamma\,(xMv)^{\frac{2}{3}} = 2{\cdot}121\,(\tau - d),$$

or—

$$\gamma\,(Mv)^{\frac{2}{3}} = \frac{1}{x^{\frac{2}{3}}} \times 2{\cdot}121 \times (\tau - d).$$

But, according to the formula we have just found—

$$\gamma\,(Mv)^{\frac{2}{3}} = \frac{K\,(\tau - d)}{1 + \mu\tau};$$

so that by equating the two right-hand sides of the equations and simplifying we get—

$$x = \left\{\frac{2{\cdot}121}{K}\,(1 + \mu\tau)\right\}^{\frac{3}{2}}.$$

The degree of association x, of methyl and ethyl alcohols, water and acetic acid, at different temperatures has been calculated in this way and the results are given in the following table :—

Methyl Alcohol.				Ethyl Alcohol.				Water.				Acetic Acid.			
t.	$\gamma(Mv)^{\frac{2}{3}}$.		x.	t.	$\gamma(Mv)^{\frac{2}{3}}$.		x.	t.	$\gamma(Mv)^{\frac{2}{3}}$.		x.	t.	$\gamma(Mv)^{\frac{2}{3}}$.		x.
	Found.	Calcd.			Found.	Calcd.			Found.	Calcd.			Found.	Calcd.	
− 89·8	361·8	369·8	2·65	− 89·8	436·1	433·3	2·03	0	502·9	503·5	1·71	20	371·2	371·1	2·13
+ 20	271·4	271·4	2·32	+ 20	331·0	331·0	1·65	20	485·3	484·9	1·64	80	—	314·8	1·92
90	196·3	198·6	2·11	100	235·0	235·1	1·39	60	446·2	446·4	1·52	140	250·2	250·0	1·72
150	131·3	128·3	1·94	160	147·2	146·4	1·21	100	403·5	403·5	1·41	200	174·9	174·7	1·53
200	60·6	63·2	1·81	220	39·2	38·0	1·03	140	357·0	355·6	1·29	280	54·8	52·9	1·30

In the table the temperatures are given under t, the observed $\gamma\,(Mv)^{\frac{2}{3}}$ under the column headed "found," the calculated $\gamma\,(Mv)^{\frac{2}{3}}$ using the constants given above, under "calculated," and the molecular complexity under x. Methyl alcohol is the only substance at ordinary temperature having a complexity much greater than 2. The molecular complexity of the other substances is usually rather less than 2, and becomes less and less as the critical point is approached.

Without going to the trouble of finding a formula which represents the variation of molecular surface energy of associating liquids with temperature, it is possible to obtain a fairly good approximation to the degree of molecular complexity from the equation—

$$\gamma\,(Mv)^{\frac{2}{3}} = \frac{1}{x^{\frac{2}{3}}} \times 2{\cdot}121 \times (\tau - d)\,;$$

but this involves the assumption that the actually found critical temperature of the liquid is not far removed from the critical temperature of the liquid, supposed to consist of simple molecules. Thus we obtain—

$$x = \left\{ \frac{2{\cdot}121\,(\tau - d)}{\gamma\,(Mv)^{\frac{2}{3}}} \right\}^{\frac{3}{2}}.$$

The results obtained in this way differ very little from those calculated by the formula—

$$x = \left\{ \frac{2{\cdot}121}{K}\,(1 + \mu\tau) \right\}^{\frac{3}{2}};$$

so that when only a few data are at hand this formula will generally be found to give a very fair idea of the molecular complexity of the liquid.

The chief result which has been gained by the investigations on the surface tensions of liquids is that liquids have been divided into two groups. One group contains the normal or unassociated liquids, the other those which form complex molecules or are associated. The degree of complexity of the associated liquids as given in the previous table is probably correct within certain limits, which are conditioned by the fact that the number chosen for the constant 2·121, is not absolutely constant but varies with the nature of the compound.

The extreme variation for the fourteen substances which have been most carefully investigated is between 2·020 for ethyl formate and 2·248 for methyl isobutyrate. On the assumption that this is the extreme divergence, there may be a positive or negative error of 5 per cent. caused by assuming the mean value to be 2·121.

In conclusion a short review of the evidence which proves in a general way that certain liquids, and especially the fatty acids, alcohols and water, possess complex molecules may not be out of place.

Guye has shown that the critical co-efficient of a liquid, that is, the quotient obtained on dividing the absolute critical temperature by the critical pressure, should, when multiplied by the constant 1·8, be equal to the molecular refraction.

Whilst the majority of substances examined by Guye appear to consist of simple molecular groups at their critical points, water, methyl alcohol and acetic acid yield numbers which point to association, inasmuch as the constant, instead of having its usual value 1·8, has decreased to about 1·1.

Young and Thomas have shown that the densities of most liquids at their critical points may be found by multiplying their theoretical densities by a number approximately equal to 3·85. Methyl, ethyl and propyl alcohols and acetic acid are, however, exceptions to this rule, inasmuch as the factor varies between four and five. This fact again points to the presence of complex molecules at the critical temperature.

In determining the true volume of water and of the above liquids at their critical points by the method suggested by Cailletet and Matthias, the same authors again find evidence of association in the liquid state.

When the numbers representing heats of vaporisation of a compound increase to a maximum and then diminish, Guye has shown from Ramsay and Young's work that the compound contains complex molecules in the liquid state. This was found to be the case with ethyl alcohol and acetic acid. The fact that the vapour pressure curve of a liquid cuts those of undeniably simple substances, such as benzene,

carbon tetrachloride, etc., may be taken as proof that the liquid contains complex molecules. Evidence of a like nature has also been brought forward by Thorpe and Rodger during their investigation of the viscosity of liquids.

This must suffice to show that between normal liquids, such as the esters, on the one hand, and the fatty alcohols and acids on the other, there exists an important difference. The characteristic difference between normal and associated liquids is, that in the case of the former the molecular surface energy is a linear function of the temperature, whilst in the latter case it is not so.

It has been assumed that the molecular weight of the normal liquids in the liquid state is the same as that of their gases. There is no positive evidence to show that the molecules of such liquids as follow the law do not associate into twos or threes on assuming the liquid state. One thing however is certain: if they do, they all associate to an equal extent, and the degree of association is not altered by rise of temperature. It is unlikely that mere liquefaction should produce in almost all cases equal association, and it is unlikely that a rise of temperature should not cause the dissociation of an associated body.

It is evident, however, that the assumption that the molecular weights of such liquids as the esters are not changed on assuming the liquid state is a mere hypothesis; but it is one for which there is a great deal of probability, and probability of the same kind as that which led to the adoption of the usually received molecular formulæ for gases.

It is now pretty generally known that the molecular weight of liquids is not the same thing as the molecular weight of liquids in liquid solution. Van't Hoff has shown that dissolved substances behave as gases, in obeying the well-known gas laws, if we only substitute osmotic for ordinary gas pressure. The molecular weight of a liquid as determined by the cryoscopic or ebullioscopic method, for example, is therefore not to be considered the same as the molecular weight of the liquid *per se*, but rather the same as the molecular weight of the substance in the gaseous

state, provided, of course, that the solvent itself has not the effect of producing molecular aggregation of the dissolved substance.

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JOHN SHIELDS.

THE ORIGIN OF THE VASCULAR PLANTS.

ABOUT forty years ago there appeared a series of publications that marked an epoch in the study of vegetable morphology. These were the remarkable papers of the German botanist Hofmeister, in which the comparative morphology of what are now known as the Archegoniatæ was discussed for the first time from the standpoint of the evolutionist. Hofmeister showed that the Mosses and Ferns belonged to one natural series, and that the Phanerogams were connected by the Gymnosperms directly to the higher Pteridophytes, and that the terms "Cryptogam" and "Phanerogam" must be discarded, at least in their old signification, as marking two radically different types of plants.

The result of Hofmeister's work was the arousing of extraordinary interest in the study of the structure and development of the Archegoniatæ and the publication of a great number of investigations upon these forms. It is not necessary to mention here the long list of eminent botanists whose names are connected with these investigations which corrected and added to the splendid results of Hofmeister's pioneer efforts. Within the last few years a still further impetus has been given to this subject by the great improvements in histological methods which have made it possible to solve many problems which quite baffled the earlier botanists. This has been especially the case in the study of the so-called heterosporous Pteridophytes, where the development of the earlier stages goes on within the spore, but applies also to the exact study of all the delicate tissues. These improvements consisted first in the application of the careful methods employed by zoologists for preserving and staining tissues, and secondly the use of the microtome for sectioning delicate organs. Undoubtedly the name of Strasburger must lead the list of those who have been concerned with the improvement of histological methods.

The study of the Archegoniatæ has an especial interest,

as they undoubtedly are the connecting links between the Flowering Plants and the simpler green Algæ which most botanists are agreed are allied to the progenitors of all the higher plants. Just which of the modern forms are most nearly allied to this ancestral type is not certain, but on the whole the genus *Coleochæte* seems to show the greatest resemblance to the simplest known Archegoniatæ.

It is the purpose of this paper to sketch briefly the process, so far as we can trace it, by which the vascular plants gradually developed from the lower Archegoniatæ, and to state the somewhat conflicting views held by different botanists at the present time as to the relationships of the principal groups of the Archegoniatæ to each other, and their connection with the Algæ on the one hand, and the Spermaphytes (Phanerogams) on the other.

In all of the Archegoniatæ there is a marked alternation of sexual and non-sexual plants which are extremely different from each other both in structure and development. In the lower members of the series, the non-sexual generation (Sporophyte) is insignificant, and physiologically is merely a spore-fruit borne upon the sexual plant or gametophyte, but in the higher ones it gradually becomes more and more an independent organism, and finally all connection with the gametophyte is severed.

The Archegoniatæ are usually divided into two groups, the Muscineæ or Mosses in the widest sense, and Pteridophyta or the Ferns and their allies. In the first series the sporophyte never becomes entirely independent; in the second it always does. Within the first, however, there is great difference in the degree of dependence of the sporophyte. In the simplest ones, *e.g.*, *Riccia*, the sporophyte is simply a mass of spores surrounded by an imperfect layer of sterile cells. In all the others, however, a greater or less amount of sterile tissue is developed, which at first serves simply to nourish the growing spores, but in the higher Mosses and Liverworts forms a true assimilative tissue, with green parenchyma and stomata quite comparable to the assimilative tissue system of the vascular plants. The resemblance is still further increased by the

presence of an axial mass of tissue (columella) which both morphologically and physiologically is probably to be directly compared to the vascular bundles of the higher plants. In the sporophyte (sporogonium) of these higher Muscineæ, only a very small amount of tissue is devoted to the production of spores, in strong contrast to the lower Hepaticæ where practically the whole sporogonium is composed of sporogenous cells.

While in a general way it may be said that the evolution of gametophyte and sporophyte in the Muscineæ is parallel, this is by no means always the case. Thus *Anthoceros*, whose sporophyte comes the nearest to an absolutely independent condition of any Bryophyte, has the gametophyte of an exceedingly simple type. In this genus, the sporophyte not only possesses an elaborate assimilative system and a central conducting tissue, but its growth is not stopped by the formation of spores, but continues as long as the gametophyte remains alive.

In the Ferns the development of the sporophyte is carried still further, and by the production of a special organ, the root, it becomes entirely self-supporting and sooner or later the gametophyte dies. The sporophyte of the Fern at first sight shows little in common with the sporogonium of the most highly organised Moss, but a more careful scrutiny shows that the differences are less than appears at first. The great difference consists in the development of special organs, leaf, root, stem, sporangia, in the former, while the compact bryophytic sporogonium in no case shows any such external differentiation.

On comparing the gametophyte of the Pteridophytes with that of the Muscineæ we find that it resembles in most cases the simpler Hepaticæ rather than the more specialised Mosses; and in the case of most Ferns this resemblance is extraordinarily close. The earlier observers noticed the very obvious resemblance of a Fern prothallium to a thallose Liverwort, and concluded that here was the point of contact between the two series of the Archegoniatæ. When, however, an attempt was made to homologise the sporogonium of any known Bryophyte with the Fern plant, the

difficulty seemed insurmountable, and more recently another theory was advanced, of which Goebel is the principal exponent at present. This theory holds that the origin of the Pteridophytes must be sought very low down in the Archegoniate series before they had assumed the characters of the modern Muscineæ, and that the two series, Muscineæ and Pteridophyta, as they exist at present, form two entirely divergent lines of development arising from common ancestors, that in structure were intermediate between the green Algæ and the simplest Muscineæ. This theory is mainly based upon the alga-like prothallium of certain species of *Trichomanes*, a genus of the Hymenophyllaceæ. The latter are Ferns, mostly of small size and extreme delicacy, mainly tropical in their distribution. In the smaller species the very delicate filmy leaves remind one strongly of the leaves of some of the larger Mosses, such as *Muium*, and this resemblance led the older botanists to suggest a possible relationship between them, and later investigators have also been led by the apparent simplicity of their structure to assume that the Hymenophyllaceæ are the most primitive of existing vascular plants.

Goebel pointed out the resemblance between the prothallium of *Trichomanes* and the filamentous protonema of most Mosses, and from a study of the former, as well as that of some of the true Mosses, especially the peculiar genus *Buxbaumia*, came to the conclusion that the primitive form from which the other Archegoniatæ have sprung was a branching alga-like filamentous form, bearing the sexual organs directly upon the filament, and that the flat thallus of the Liverworts and most Fern prothallia is a secondary development, the two having nothing in common. Unfortunately Goebel does not offer any explanation of the origin of the sporophyte of the Hymenophyllaceæ either from forms such as exist at present or from hypothetical ones, but confines his attention almost exclusively to the sexual generation. This one-sided view of the question weakens very much the force of his arguments, and serious objections to his theory arise as soon as the subject is considered from other directions. Even putting aside the

structure of the sporophyte, the comparison of the filamentous prothallium of *Trichomanes* with the protonema of a Moss is hardly convincing, as the former is homologous not with the protonema alone, but with the protonema *plus* the leafy stem, and the Moss *Buxbaumia*, which he considers the most primitive in that the leafy stem is extremely reduced, is known to be saprophytic, and this abnormal habit is quite sufficient to account for the great reduction of the vegetative organs of the gametophyte.

While the filamentous prothallium of some species of *Trichomanes* is certainly strikingly like a Moss protonema, other species have the flat prothallium like that of other Ferns, and this is always the case with normally developed prothallia of all species of the closely-relative genus *Hymenophyllum*. These differences might be explained if we had to consider only the prothallium, but when in addition the character of the sporophyte is taken into account it becomes impossible to reconcile the radical differences between the simplest forms of the Hymenophyllaceæ and any known Bryophyte.

The forms of the Bryophytes whose protonemata most resemble the prothallium of *Trichomanes* are all specialised types whose sporogonia are very far removed indeed from the simple character that would be expected in primitive forms, while all the Bryophytes whose sporogonia can in any way be considered primitive are thallose, and much more like the flat prothallium of the majority of Ferns; this, in connection with the great similarity in the method of growth of the two, makes it seem much more likely that the older view of the origin of the Ferns from forms like the thallose Hepaticæ is correct. This seems the more probable as a study of the sexual organs shows much in common between some of the thallose Liverworts and certain Ferns, while all the true Mosses differ much from the latter in this respect.

Botanists at present usually divide the Ferns into two main divisions, the Eusporangiatæ and the Leptosporangiatæ, the distinction being based upon the character of the sporangia. In the latter group, which comprises an

enormous majority of all existing Pteridophytes, the sporangia are derived from a single epidermal cell, and in all cases provided with a curious ring of thickened cells, the annulus. In the Eusporangiatæ the sporangia are much more massive and multicellular from the first, and the annulus is extremely rudimentary or usually entirely absent. The latter group is unquestionably nearer to the seed plants in the structure of the sporangium, and this has been the cause of their being regarded as higher in the series than the Leptosporangiatæ which are connected with them by almost insensible gradations; and it is only quite recently that it has been suggested that the Eusporangiatæ, while doubtless related on the one hand more nearly to the flowering plants, are at the same time nearest the Bryophytes. This hypothesis considers the Leptosporangiatæ as a comparatively modern group of plants, derived from the Eusporangiatæ, which are also the ancestors of a portion, at least, of the seed plants. If this view is correct the Eusporangiatæ are an ancient generalised type connecting the Bryophytes directly with the higher vascular plants, and from them as a starting-point have diverged two, and possibly more, distinct lines of development, one of which has terminated in the most specialised of the Leptosporangiatæ, and another in the Angiosperms. According to this theory the Hymenophyllaceæ, instead of representing the most primitive group of vascular plants, are really a somewhat specialised one, whose peculiarities are largely the result of adaptation to a peculiar environment, all of them being inhabitants of dark moist localities to which their delicate structure is especially adapted. In their essential characters they show most evidently a close relationship with the other Leptosporangiatæ and no suggestion of structure comparable to that of the Bryophytes.

The more carefully the Eusporangiatæ are examined, the stronger becomes the conviction that they are really primitive forms connected with the higher Bryophytes. This is true both as regards the prothallium and the sporophyte. The former reaches among these a degree of

development not found elsewhere, and resembles in a most remarkable degree that of many thallose Liverworts. This is especially the case with the sexual organs which are much like those of *Anthoceros*, whose sporogonium has long been recognised as the nearest in structure among the Bryophytes to the Ferns. This resemblance of the sporophyte in the two, is more than a general one. Not only is the development of spores in *Anthoceros* subordinate to the vegetative growth of the Sporophyte, but the origin of the sporogenous tissue is directly comparable to that in the eusporangiate Pteridophytes. In both cases the sporogenous tissue (Archesporium) is derived from hypodermal cells, and the segregation of the sporogenous cells into definite sporangia is already indicated in the Anthoceroteæ, although the sterile cells do not form firm walls between the masses of spores. Of the living Ferns, *Ophioglossum* seems the nearest to *Anthoceros* so far as the sporophyte is concerned. The sporangial spike of the former may be directly compared to the upper part of the sporogonium of *Anthoceros*, from which it differs mainly in the complete separation of the archesporial cells, so that a number of separate cavities are formed, constituting the large sporangia of this genus. In some species, *i.e.*, *O. vulgatum*, the epidermis above the masses of spores develops stomata, as in *Anthoceros*, and as in that form the sporogenous cells are hypodermal and the outer wall of the sporangium at first but a single layer of cells, which later becomes much thicker. The sporangia project very little, and compared with other Ferns are large. The genus *Botrychium* contains a number of species by which *Ophioglossum* is connected with the *Leptosporangiatæ.* There is a gradual increase in the definiteness of the sporangium, which becomes more and more raised above the surface of the leaf until in the more specialised forms, *e.g.*, *Botrychium Virginianum*, it is a globular capsule raised upon a short stalk. As the sporangium becomes more distinctly separated from the leaf, it becomes smaller, with a corresponding increase in the number of individual sporangia. Thus while in *Ophioglossum* the fertile leaf segment forms a

spike with only two rows of sporangia, in the larger species of *Botrychium* it is a compound panicle with very numerous, but very much smaller sporangia. This increase in the number of sporangia is correlated with a corresponding division of the sterile segment by the leaf which becomes very ample and much divided.

The type of sporangium in *Botrychium* is very much like that of the Osmundaceæ, which in turn are closely connected with the true leptosporangiate Ferns ; but whether there is any genetic relation between *Botrychium* and *Osmunda* is another question, which at present we are not in a position to answer. In these forms, as well as in all the typical Leptosporangiatæ, the archesporium is readily traceable to a single cell, and in the latter group the whole sporangium is derived from a single epidermal cell, which is never the case in the Eusporangiatæ. In all the Leptosporangiatæ the wall of the sporangium is reduced to a single cell in thickness, and is usually long-stalked, especially in the Polypodiaceæ, which are probably the highest of the homosporous Ferns.

The very complete series of forms leading from the Leptosporangiatæ to the Eusporangiatæ has often enough been commented upon, but until the recent attempts to prove the primitive character of the latter it was taken as a matter of course that the Leptosporangiatæ formed the beginning of the series, and not the end. The objections to this view may be stated briefly as follows. First, the type of sporangium of the Leptosporangiatæ is very specialised, while that of the Eusporangiatæ approaches the condition found among the Bryophytes ; second, the prothallium of the Eusporangiatæ approaches much nearer in structure the thallus of the Hepaticæ, both in its growth and in the structure of the sexual organs, and the embryo is much longer dependent upon it ; thirdly, the geological record shows conclusively that the Eusporangiatæ were the prevailing forms among the Carboniferous and pre-Carboniferous Ferns, and the Leptosporangiatæ only appeared later, increasing in number and variety in the more recent strata.

The method by which a simple Fern like *Ophioglossum* may be supposed to have arisen from a structure like the sporogonium of *Anthoceros* would be somewhat as follows. From the large "foot" or absorbent organ at the base of the sporogonium of the latter, by further growth an organ might be formed piercing the thallus and penetrating into the ground. It is extremely probable that in some such way as this the first root of the vascular plants originated, and so soon as this took place the sporophyte would be quite independent of the gametophyte. The next step would be the development of a special assimilating organ, the leaf, which might be expected to arise as an outgrowth from the zone of active cells between the foot and the capsule in such a form as *Anthoceros*, which would then strikingly resemble a small *Ophioglossum*. The vascular bundles, which of course are absent, at least in their complete form, from the sporogonium of all Bryophytes, are undoubtedly directly comparable to the central strand of vegetative cells which forms the columella of *Anthoceros*, and is also found in the sporogonium of most Mosses. Even in these forms this central bundle of cells probably acts as a conducting tissue, although the different kinds of tissue found in the bundles of the true vascular plants are not yet differentiated.

In comparing *Anthoceros* directly with such Pteridophytes as *Ophioglossum*, it must not be supposed that a direct connection between the two is assumed. There are some differences so great as to forbid this. Thus the extremely peculiar chlorophyll bodies of all the Anthoceroteæ are quite confined to this order, and are very different from those of the Ferns. The spermatozoids, too, have but two cilia, instead of the numerous ones found in all true Ferns. However, in spite of these differences, the resemblances are so striking that it is safe to assume that the ancestor of the Pteridophytes was probably a liverwort-like plant having a sporogonium essentially like that of Anthoceros.

The latest theory as to the method of origin of the Pteridophytes is what may be called the "Strobiloid Theory".

This assumes that the forms like *Lycopodium* and *Equisetum*, in which the sporophyte forms a strobilus, or cone, are the most primitive. Professor Bower, who has advanced this theory, bases his conclusions upon a most thorough study of a very large number of forms, and his arguments are very convincing, at least so far as concerns the really strobiloid forms. In regard to the Ferns, there are some serious difficulties in the way. This theory assumes, as does the one just considered, that the ancestor of the Pteridophytes was a form like *Anthoceros*, with a continuous hypodermal archesporium, and that this in turn was derived from simpler forms in which the sporogenous tissue was much more prominent. The first step in the development of the strobilus was the segregation of the sporogenous cells by masses of sterile tissue, exactly as we have supposed in the evolution of *Ophioglossum*. Instead, however, of this sterile tissue simply forming the partitions between the sporangia, it is supposed to have developed into outgrowths or leaves, so that corresponding to each leaf there was a separate sporangium such as is found in *Lycopodium*. The form which Professor Bower considers to come nearest the primitive condition is a curious little lycopodinous plant, *Phylloglossum*. This interesting form shows certain striking resemblances to the embryo of various species of *Lycopodium*, the common club mosses, and seems to have retained its primitive or embryonic condition. From such a type as this the other Lycopodineæ are supposed to have descended.

Professor Bower is inclined to regard all of the Pteridophytes as reducible to a strobiloid form, but as far as the Ferns are concerned there are very serious difficulties in the way. Aside from apparently essential differences in the sporophyte between Ferns and Lycopods, there are also differences in the gametophyte which cannot readily be explained on the ground of a common origin. The most essential difference is in the spermatozoids. These in the Lycopodineæ are bi-ciliate like all known Muscineæ, while in the Ferns and Equiseta they are multi-ciliate. It is quite conceivable, nay, probable,

that there have been two distinct lines of development, perhaps more, among the Pteridophytes, arising quite independently of one another, but from similar ancestral forms. In one of these, the Lycopodineæ, the leaves are small, the axis much elongated, and regularly each sporophyte bears but a single sporangium. In the other, the Filicineæ, the leaves are large and the axis usually much less developed; sporangia are found usually in large numbers upon the, sometimes, profoundly modified sporophylls. Whether the third group of living Pteridophytes, the Equisetineæ, form a third entirely distinct series, or whether they are more nearly related to one or the other of the two other Classes, is by no means easy to determine.

The parallelisms in development found in the three groups do not necessarily imply a genetic connection, as perhaps the most remarkable case of all, that of heterospory, has beyond question arisen independently in at least five widely remote groups where the question of relationship cannot for a moment be maintained. The development of heterospory in unrelated groups of Pteridophytes also suggests the possible multiple origin of the "Flowering Plants," or, more correctly, "Seed Plants". That heterospory was the first step in this process no botanist would think of disputing, and the homologies of the macrosporangium of the Pteridophytes and the ovule of the Phanerogams are universally admitted, as well as those of the microspores and pollen cells. When, however, the question is raised as to which group of Pteridophytes has given rise to the Phanerogams, and whether all of the latter belong to a common stock, the matter is not so simple. Hofmeister first showed that the homologies between the Gymnosperms and the heterosporous Lycopodineæ were very striking, and that the former, except in the absence of spermatozoids, were really nearer to the Pteridophytes than to the Angiosperms. It was assumed that the latter were derived from the Pteridophytes through the Gymnosperms and that the Phanerogams formed a single series; but later many botanists have dissented from this view and are inclined to look for the origin of

the Angiosperms directly from some group of Pteridophytes. The peculiar genus *Isoëtes* has been suggested as a form possibly resembling this ancestral type, but it is still an open question to which group of homosporous Pteridophytes *Isoëtes* is most nearly related.

The essential points of structure of the Phanerogams, the seeds and pollen tube, are only further developments of heterospory, and may very readily be conceived to have arisen independently more than once. The structure of the Angiosperms is too constant to make it probable that the Monocotyledons and Dicotyledons are not intimately related, but among the Gymnosperms the diversities of structure are so great that it is quite possible that the Conifers and Cycads, for example, may not be at all related, but constitute two quite separate lines of development.

RECAPITULATION.

To sum up then what we have endeavoured to state in the foregoing pages, it is generally admitted that the origin of the vascular plants is to be sought among the less specialised Bryophytes, which in turn are unquestionably derived from algal ancestors. There is, however, diversity of opinion as to whether the first vascular plants came from forms similar to those existing at present, or from forms immediately between them and the Algæ.

It is further admitted that in the evolution of the sporophyte it gradually passed from a condition where its whole substance was devoted to spore-formation to that where more and more spore-formation was subordinated to the vegetative life of the sporophyte. This now, by the development of special organs, roots and leaves, became entirely free from the gametophyte upon which in the lower forms it had lived as a parasite. Of the living Bryophytes, or non-vascular Archegoniatæ, the genus *Anthoceros* comes nearest to realising this condition.

From a condition like that found in *Anthoceros* it is quite probable that more than one line of development has proceeded, resulting in the different groups of Pteridophytes, which in their turn may have given rise to seed-bearing plants quite independently of one another.

DOUGLAS HOUGHTON CAMPBELL.

RECENT RESEARCHES IN THERMAL METAMORPHISM.

PART II.

SOME interesting instances of metamorphism in various impure calcareous rocks (calc-mica-schists) have been described by Weinschenk from the Venediger *massif* in the East Central Alps (19). The igneous intrusions that have produced the transformations have been almost pure pyroxene rocks, and are now represented by serpentine rocks showing many features of interest. In contact with the serpentine at the falls of the Islitz is an "omphacite-like" rock consisting of tremolite, grey limestone, etc., with infiltration of serpentine substance. This zone is sometimes several metres in width, and is succeeded by a complex of crystalline silicate rocks, which sometimes recall eclogites and allied types, and are true lime-silicate-hornfels of various constitution. In part they are characterised by garnet, but nearer to the contact epidote and diopside occur instead, besides a blue amphibole and apparently a plagioclase felspar. The garnets often show double refraction and zonary growth, a character frequently found in the garnets of metamorphosed calcareous rocks in different areas. In other localities described by Weinschenk the metamorphosed calcareous schists have the character of "epidote-hornfels" (with plagioclase, epidote, and actinolite), "diopside-hornfels," etc. Whether these observations throw any light on the origin of eclogites and similar rocks may be doubted, since, as pointed out by Rosenbusch, the typical eclogites of Central Europe agree in chemical composition with true igneous rocks. It is certain, however, from observations in other districts, that the monoclinic pyroxenes produced in thermal metamorphism include omphacites, *i.e.*, varieties rich in alumina and lime, as well as the non-aluminous diopsides, etc.

As already intimated, under the conditions which obtain

during thermal metamorphism, the carbonates are not decomposed except in presence of silica in some available form to replace the carbonic acid. Consequently the most striking effects are to be observed in limestones or dolomite rocks containing a large percentage of impurities and in calcareous shales, tuffs, etc. Here the carbonic acid is entirely expelled, and rocks are formed consisting in great part or wholly of lime-bearing silicates. Such a rock—the Kalksilicathornfels of the Germans—is often of very complex constitution, and may be of so fine a texture as to require very minute study. Of this type are the compact porcellanous-looking rocks consisting essentially of a fine mosaic of augite, wollastonite, felspar, etc., sometimes enclosing rather larger crystal plates of some pyroxene, nests or patches of tremolite needles, etc. On the other hand there are lime-silicate rocks of which the constitution is more simple and manifest, such as the type which consists of dodecahedra of lime-garnet set in a matrix of crystalline idocrase. These and other types are found in various beds of the Coniston Limestone group, where they are metamorphosed by the Shap granite (3), and in these rocks English geologists may study almost all the phenomena that have been described in the classic districts of the Harz and the basin of Christiania.

A survey of the recorded facts regarding the lime-silicate rocks leads us to two or three general conclusions. In the first place we note that quite a moderate proportion of calcareous matter in a rock, which would not cause a field geologist to style it a limestone, is sufficient to make the metamorphism follow this line, and that such a rock, in contrast with a purer limestone, has its carbonic acid completely eliminated in the process of metamorphism. Further, we see that such changes in impure calcareous rocks are quite readily induced, the lime-bearing silicates apparently demanding no very elevated temperature for their production in this way : this fact seems to connect itself with the rather easy fusibility of the minerals in question, though the conditions of metamorphism are, of course, different from those of dry fusion. Lastly, the great variety in

mineral constitution of the rocks produced and the complexity of some of them are very striking. Calcareous beds which seem to differ but little may give rise by metamorphism to quite different mineral aggregates. Presumably slight differences in the chemical composition of the rocks affected may determine wide diversity in the recombinations set up in them, thus emphasising the prime importance of the bulk composition as affecting the results of the metamorphism.

It is only in recent years that any detailed study has been made of thermal metamorphism in *rocks of igneous origin*. The results promise to shed considerable light upon the problems offered by large areas of crystalline schists. It may be laid down as a broad rule that fresh igneous rocks are much less susceptible to changes by thermal metamorphism than are most ordinary sediments. This is a simple consequence of the fundamental principle of these mineralogical transformations, *viz.*, the instability of chemical compounds under changed physical conditions. Substances such as the common weathering products, formed at ordinary atmospheric temperatures, become unstable when subjected to much higher temperatures; but minerals which have originated from igneous fusion are, as a rule, much less liable to change when heated. In both categories there are a few exceptions, *e.g.*, quartz under the former head and the pyroxenes under the latter, but the rule is of general application.

Conversely, combinations entered into at high temperatures, as in rocks formed from fusion, cease to be stable at ordinary low temperatures; and most igneous rocks which are not geologically very young have suffered more or less change from this cause. The transformations induced in them are further connected with the access of meteoric water, carbonic acid, oxygen, etc., as is evident from the nature of the common "weathering" products. Igneous rocks so altered are quite as readily affected by thermal metamorphism as argillaceous or other sediments, the first signs of change appearing always in the decomposition products of the rocks.

The earliest observations of thermal metamorphism in igneous rocks were made by Allport (1876) on certain diabases near the Land's End granite, and similar phenomena have been described by Lossen in the Harz and by other geologists in several other districts. The chief features noted were the conversion of augite to green hornblende, and the recrystallisation of the felspar; and these are found everywhere to be highly characteristic changes. Brown mica may be formed with, or instead of, hornblende, but, in some cases at least, this seems to arise not directly from the augite but from its chloritic decomposition products. Other basic rocks show like transformations. The gabbro metamorphosed by the Carrock Fell granophyre in Cumberland has developed green actinolitic hornblende and brown biotite (20). The latter occurs chiefly around the grains of iron ore, from which it has probably taken up some ferrous oxide and titanic acid. At the same time the turbid felspars have become clear. Callaway (21) has described the metamorphism of diorites in the Malvern range and in Galway. In the former case the hornblende, perhaps previously weathered, has given rise to deep brown biotite in the vicinity of an intruded granite, while white mica has been produced from the plagioclase felspar. In the other case the plagioclase has been simply recrystallised, while a chloritic mineral, epidote, and rarely biotite, represent the original hornblende. In an altered diorite on the western flanks of Dartmoor McMahon (22) finds a red mica as the characteristic secondary mineral, most of the hornblende having disappeared.

On the metamorphism of volcanic rocks we have fuller information. Besides the cases already noted in Saxony, there are most instructive examples in the English Lake district, especially around the Shap granite, where lavas and tuffs of acid, intermediate, and basic composition may be studied in various stages of alteration (3). One general conclusion there brought out is confirmed elsewhere, *viz.*, that the acid rocks are commonly much less liable to metamorphism than the more basic. This is doubtless due to their more simple chemical composition. The rhyolites

close to the granite often show little or no change that can be certainly attributed to heat, and the rhyolite fragments in a highly altered calcareous breccia retain all their minute structures—cryptocrystalline, microspherulitic, and perlitic—in perfection. Where, however, the rocks before metamorphism contained decomposition products, these have given rise to new minerals, the characteristic green pinitoid substance being converted to a mixture of white and brown micas. In the acid tuffs, too, which were more composite and more weathered than the lavas, various metamorphic minerals have been set up.

The augite-andesites of the same district had been extensively weathered prior to metamorphism. Chloritic minerals, calcite, chalcedony, and quartz had been formed from the pyroxene and felspar, and were partly disseminated through the mass, but largely collected in little veins and in the abundant vesicles of the rocks. These weathering products were the parts most readily affected by heat. The chloritic mineral has been converted into biotite, or, where it was associated with calcite, into green hornblende, which specially characterises the metamorphosed amygdules: chalcedonic silica has been transformed into crystalline quartz. Nearer the granite the lavas are more altered, sphene, magnetite, pyrites, etc., being formed; the porphyritic felspars are replaced by a mosaic of new felspar substance; and finally the whole mass of the rock is reconstituted into a fine-textured ground.

A more basic type of lava shows similar phenomena, but, in consequence of the higher percentage of lime present, the new-formed minerals are not quite the same. Green hornblende here predominates over biotite, and an augite has also been formed, especially in the amygdules and veins. Another characteristic mineral is epidote. Some large amygdules show a complex association of lime-bearing and other metamorphic minerals—garnet, hornblende and actinolite, augite, quartz, sphene—and, in the centre of the largest ones, some residual calcite recrystallised but not decomposed. Another type of basic lava—the well-known hypersthene-bearing rock of Eycott—is

metamorphosed by the Carrock Fell gabbro, the bastite pseudomorphs after hypersthene being converted into hornblende (20).

The basic and intermediate tuffs near the Shap granite behave in general very like the associated lavas, though with some differences. Brown biotite is always the most conspicuous mineral in the metamorphosed rocks, and gives them the character of mica-schists or fine micaceous gneisses. A marked foliation is usually evident, the mica flakes being developed parallel either to the bedding or the cleavage of the tuffs, and a similar foliation often follows the flow lines of the lavas. The metamorphism of some Carboniferous volcanic tuffs on Dartmoor has been described by McMahon, the chief point of interest being the production of anthophyllite, a mineral not yet recognised in the Shap district. It occurs in radiating bundles of needle-like crystals with the usual characters (22).

But little detailed information has been collected relative to thermal metamorphism in the various types of *crystalline schists* and allied rocks. Among contributions to this subject in recent years the late G. H. Williams' account of the metamorphosed rocks on the Hudson River (23) is of special interest. It illustrates the changes set up in a series of mica-schists invaded by massive intrusions of diorite. Approaching the intrusive rock, the mica-schists are found to become more and more contorted and filled with "eyes" or lenticles of quartz containing garnet and other "contact minerals"—a feature noticeable in the metamorphosed flags of the New Galloway district already mentioned. In the mica-schists themselves have been developed staurolite, sillimanite, cyanite, and garnet, the completeness of the metamorphism increasing in the direction of the contact, where the schistose character is wholly lost and the rocks become hard and massive. In the progressive stages of metamorphism observed a gradual disappearance of the quartz and muscovite of the schists is found to accompany the development of biotite, sillimanite, staurolite, and garnet. This points to a chemical as well as a mineralogical change in the composition of the rocks, and indeed analyses show

a decrease in the proportions of silica (from 63 to 40 per cent.) and of alkalies and an increase in the proportions of alumina and iron oxides. The author does not assume that the rocks analysed were before metamorphism of identical composition, but making allowance for original differences, he claims that there has been a loss of certain ingredients or an addition of others. It is clear, however, that such rocks as mica-schists are not suitable for a strict inquiry on this point: we require rocks in which an individual bed can be followed into the metamorphic aureole, so that the material studied may be fairly assumed to have been of uniform character before metamorphism. The same objection applies to the conclusions which Hawes at an earlier date drew from the metamorphosed mica-schists near the Albany granite.

Some curious observations were made by Williams upon inclusions of highly metamorphosed schist in the mass of the intrusive diorite. Of these some consist mainly of pleonaste and corundum, with some magnetite, biotite, and felspar; others are essentially of quartz grains with various accessory minerals; others of staurolite and ripidolite with more or less sillimanite; and others again largely of flakes of margarite with varying amounts of other minerals. The phenomena bear a close resemblance to those seen in the neighbourhood of Klausen in the Tyrol, and the remarkable mineral aggregates constituting the inclusions are also paralleled by the included patches in the kersantite of Michaelstein in the Harz, described by Koch (24). The occurrence of minerals very rich in alumina, such as pleonaste and even corundum, is a curious feature in all the three districts.

An exhaustive memoir by Salomon deals with the thermal metamorphism of various crystalline schistose rocks in the Adamello range, between Italy and the Tyrol (25). The intruded rock which causes these transformations is the well-known "tonalite". Certain phyllites in the earlier stage of metamorphism show especially the production of brown biotite; nearer to the contact they pass into a schistose andalusite-mica rock consisting of muscovite,

quartz and biotite, with scattered crystals of andalusite and in certain layers blue grains of corundum. The intrusive rock itself contains inclusions, some similar to the neighbouring metamorphosed schists, others different and especially rich in cordierite.

More remarkable is the metamorphism of a lower group of gneisses, mica-schists, etc. The minerals composing the altered rocks are cordierite, andalusite, biotite, muscovite, quartz, and felspars, with sometimes garnet, tourmaline, sillimanite, spinel, corundum, and other minerals. Some of these are original constituents of the rock, such as ordinary orthoclase and muscovite, which disappear with further metamorphism; the rest are of secondary origin. In the inner zone of alteration, extending 100 or 150 metres from the contact, the rocks have lost all gneissic and schistose structures. The dominant type contains 60 or 70 per cent. of cordierite with biotite, andalusite, quartz, sillimanite, ilmenite, zircon, etc. The tonalite itself has inclusions which are taken to represent partially melted fragments of the adjacent rocks. They are rich in cordierite, but contain in addition spinel and corundum.

Phenomena more or less resembling those described by Salomon have been noted by Beck and Schalch in the gneisses and mica-schists of Saxony (26). The metamorphism of mica-schists has also been described by Müller from Schneekoppe, the highest mountain of the Riesengebirge, where the rocks abut upon an intrusive mass of granite (27). Here andalusite is the most conspicuous new mineral formed, in prisms several centimetres in length. The filmy layers of pale potash-mica of the original schists gives place to a dark magnesian variety in isolated flakes, the quartz occurs in isolated grains instead of in lenticles, and the schistose structure of the rock becomes much less marked. Further, the imperfect crystals of garnet full of inclusions are replaced by clean lustrous crystals with good forms. The metamorphism here is clearly much less intense than in the district surrounding the great tonalite intrusion of the Adamello Mountains.

It will be noticed that the most extreme metamorphism

is usually found in portions of the "country" rocks caught up as *inclusions* by intruded magmas, and the reason of this is easily understood. Moreover, in these included patches, if anywhere, we may look for evidence of actual permeation by the igneous magma, and consequent change in the bulk-composition of the fragments affected. Unfortunately in such cases we cannot always have certain knowledge of the original nature of the included rock before its transformation. Among a number of descriptions of such inclusions we may note that given by K. Vogelsang of the curious patches enclosed in the andesite of the Bocksberg and of Rengersfeld in the Upper Eifel (28). The constituent minerals are cordierite in polysynthetic crystals, andalusite in good prisms, sillimanite needles, felspar (mostly triclinic) in very variable amount, flakes of biotite, octahedra of green pleonaste, tabular crystals of corundum, etc. Very various associations of these minerals occur, and the relation of the inclusion to the matrix also varies. The general resemblance of these inclusions to what has been observed in the thermal metamorphism of some crystalline schists is evident, the abundance of aluminous minerals being again very striking. For instance, one inclusion, composed largely of sillimanite, gave about 47½ per cent. of alumina. Precisely similar inclusions occur in the andesite of the Wolkenburg and the trachyte of the Drachenfels in the Siebengebirge, while some of the volcanic ejectamenta of the Laacher See also present many points of resemblance. Vogelsang gives reason for regarding them all as fragments of crystalline schists caught up in the igneous magma, which has partially melted them and given rise to new minerals. Some of the cordierite and the pleonaste are certainly newly formed, and the author has artificially imitated the phenomena by placing a fragment of cordierite-gneiss in melted andesite.

By a careful study of the inclusions in certain Scandinavian diabases, Bäckström (29) has succeeded in making out something of the manner in which they have been modified by the enveloping magma. This case differs, however, from the others mentioned above in that the

enclosed fragments were originally of granite. The ferro-magnesian minerals of this rock, mica and hornblende, have been dissolved and absorbed by the diabase magma, rarely leaving any trace. The felspar crystals, chiefly microcline, have been attacked at the edges and penetrated along countless small cracks, which gave the basic magma access to the interior, where hollows have been dissolved out. The quartz grains of the granite have been only externally corroded and are thus rounded. The magma has given rise within and around the old microcline crystals to prisms of an intermediate soda-lime-felspar, in parts intergrown with quartz, and the effects produced may be likened in some respects to those described in the large felspar crystals of the Shap granite where they are enclosed in the dark and relatively basic portions of that rock (3).

The permeation or injection of a rock by an igneous magma, which we are here asked to consider, is clearly something more than mere thermal metamorphism, and must be essentially a *contact* action. It seems doubtful whether it has operated, except on a very restricted scale, at any ordinary junction of a metamorphosed rock with an intruded magma. Sollas has described it at the contact of a gabbro with a later intrusion of granophyre in the Carlingford district (30). The latter rock has penetrated the former in dykes and veins of all gradations of size down to microscopic films and specks occupying minute cracks and cavities in the gabbro, and the author seems to be of opinion that quartz-bearing gabbros in general have originated in a similar fashion. At Carrock Fell, however, where somewhat similar relations have been investigated by the present writer, such a process of injection seems to have been confined to a very narrow zone along the actual junction of the gabbro and granophyre, while the main body of quartz-gabbro owes its origin to a special kind of differentiation in the gabbro magma while still fluid (20). In the Irish case the details given of the extent and mode of occurrence of the rock types described are scarcely enough to warrant any expression of opinion on this point. Sollas has also described, in County Wicklow, the transformation

of a hornblende rock (amphibolite) into a rock having the mineralogical constitution of a quartz-mica-diorite. Traced from east to west, the rock undergoes a remarkable change, quartz and orthoclase as well as plagioclase-felspar being developed, while simultaneously the hornblende becomes actinolitic, and gives rise to much black mica. This change is ascribed to numerous veins of quartz and potash-felspar, which invade the rock on its western side.

The question, to what extent an actual *addition of material* may take place from an intruded igneous magma to the adjacent rocks metamorphosed by it, is one that has been much debated. Although the actual fact of such transference seems to be established in certain particular cases, the conditions which admit of it, and which limit its operation, are still very obscure. The commonest case is that in which the metamorphosed rocks near the junction have received an accession of certain *volatile* constituents, particularly boric and hydrofluoric acids. The characteristic minerals produced are tourmaline, topaz, axinite, fluorite, certain varieties of white mica, etc., and these minerals may occur in the marginal part of the intrusive rock as well as in the metamorphosed products. Such phenomena are well known in many districts, and seem to be specially connected with intrusions of certain thoroughly acid rocks: the Cornish granites afford good examples.

The difficulty arises, however, in connection with substances supposed to have been introduced *in solution* from an intruded magma and to have impregnated the adjacent rocks. The "contact" rocks known as adinoles in the Harz and other districts apparently give evidence of a notable addition of soda and silica. The rocks bordering Monzoni augite-syenites also show an addition of material, and other cases might be cited. On the other hand, the mica-schists described by Williams at Peekskill on the Hudson River seem to show a falling off of silica and alkalies as the junction is approached (23). An apparent falling off in certain chemical constituents may, of course, conceivably be caused by an increased proportion of other constituents of the rock. We naturally look, however, for

some relation between the particular constituents added to the metamorphosed rocks and the nature of the magma that furnished such constituents, and here the facts seem very difficult of explanation. The observed enrichment in silica and alkali is found not near intrusive rocks rich in those constituents, but especially in contact with diabases.

Brögger, as a result of his earlier researches in the Christiania district, laid down the rule that the phenomena of metamorphism are of the same kind, whatever the nature of the intrusive rock that has produced them. Recently he has found cause to modify this conclusion (31). At Sölvsberget, in the neighbourhood of Gran, the argillaceous shales with Ogygia are metamorphosed by a considerable intrusion of a basic rock described as olivine-gabbro-diabase. They are altered first into dark violet "hornstones" with increasing development of minute flakes of mica, and then into a visibly crystalline rock enclosing crystals of plagioclase-felspar more than 5 cm. in length. Near its contact with the basic intrusion this highly altered rock contains a considerable amount of hypersthene, and the magnesia and ferrous oxide of this mineral are supposed to have been obtained from the intruded magma. The same shales where metamorphosed by a more acid rock—the nordmarkite or quartz-syenite of the district—do not contain the exceptional mineral in question. The author writes guardedly, and the evidence is clearly incomplete without bulk analyses of the altered and unaltered shales.

Another question of considerable interest is that of the *interchange of material* within the mass of a rock undergoing thermal metamorphism, apart from any accession of matter from without (32). The very variable characters of many metamorphosed rocks and the preservation of such structures as the spherulitic, the amygdaloidal, and the laminated seem to indicate that any redistribution of material in the rocks during metamorphism is confined to very narrow limits. In other words the new minerals produced at any point seem to depend on the original chemical composition of the rock in the immediate neighbourhood of that point. In this connection the metamorphosed amyg-

daloidal lavas near the Shap granite are especially instructive (3), (32). These contain large amygdules, up to two inches or more in length, which before metamorphism were filled by decomposition products, including calcite, a chloritic mineral, and quartz. The calcite seems in general to have occupied the central part of the vesicle. As already mentioned, these substances have entered into new mineral combinations, among which lime-bearing silicates figure prominently; but in the centre there remains some calcite undecomposed. The manner in which this mineral is penetrated by needles of actinolite, etc., proves that it must have recrystallised during the metamorphism, and clearly its temperature must have been the same as that of other calcite in the same vesicle, which has been destroyed to form garnet, augite, etc. We have already remarked that in thermal metamorphism calcite is decomposed only in the presence of available silica to replace the carbonic acid driven off, and we now see that silica does not travel for this purpose more than a very small fraction of an inch. Results quite confirmatory of this extremely narrow limit to the migration of material are obtained from a study of calcareous nodules in metamorphosed shales, of the "spots" in altered slates, and of the dimensions of individual crystals of various metamorphic minerals. Whatever part water may play as a solvent in thermal metamorphism, it apparently does not act to any appreciable extent as a carrier to facilitate interchange of material between different parts of a rock.

In the opinion of the present writer the idea of solution can only be entertained in this limited sense, the action of any solvent or flux supposed to be involved being in the strictest sense local. We have hitherto discussed no speculations as to the processes by which new mineral combinations are set up during thermal metamorphism, and crystals of considerable size formed in the heart of what might be regarded as a solid rock. The question is one to which investigation has scarcely been directed by most inquirers. Hutchings (9) has recently approached it, his point of view being such as leads him to lay stress on the solvent action

of water at high temperatures, and to regard the new-formed minerals as recrystallised from such solution. He has made a special study of the amorphous matter, without action on polarised light, which occurs in many of the metamorphosed slates, etc., examined by him, and often plays an important part in such "spots" as do not consist of definitely individualised crystals. He finds gradations from the perfectly isotropic matter in question, through indefinite cryptocrystalline aggregates, into finely divided mica with other substances not certainly recognisable. The isotropic substance he believes to represent the result of relatively rapid cooling of the supposed solutions, while definite crystallised minerals represent, at the other extreme, the result of slow cooling. Further he conceives such solutions formed in the inner and intensely hot zone of metamorphism to be capable of permeating the outer and cooler zones and depositing there the dissolved material. In this particular his views are at variance with some of the conclusions which seem to follow from the observations of others. The conception of bodily solution and recrystallisation going on in a rock mass, which nevertheless retains not only its form but many of its minor structural characters, is one which confronts us with obvious difficulties, and we must be content to await further enlightenment as to the *rationale* of the complex processes involved in the mineralogical transformations of rock masses.

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CONTINUOUS-CURRENT DYNAMOS.

PART II.

IN the preceding article, the question of the influence of the current in the armature of a dynamo upon its magnetic field was briefly discussed, and it was shown that later theorists had verified and expanded the original treatment of the subject by the Drs. Hopkinson in their paper on "Dynamo-Electric Machinery". Again we have to refer to the same paper as containing the germ of a recent remarkable invention, an account of which is reserved for the present article.

In 1886, the Drs. Hopkinson described an experiment, in which a machine was run as a generator without any use of its magnet winding in order to excite its field. The brushes were given a backward lead, *i.e.*, were shifted to the position, *no*, Fig. 3, which they would ordinarily

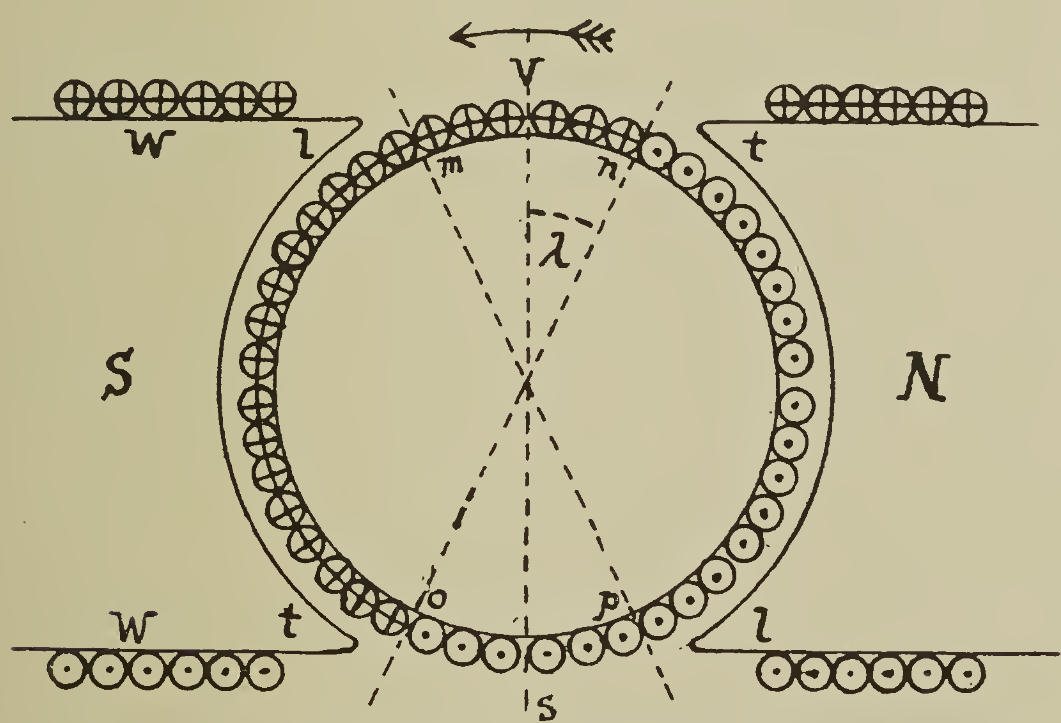


FIG. 3.—Forward and cross Ampère-turns of Armature with negative lead of brushes.

occupy in a motor; and when the circuit was closed, the residual magnetism of the iron caused a certain E. M. F. to be generated in the wires under the pole-pieces. A current was thus set up, and on comparing Fig. 3 with Fig. 2 of the last article, it will be seen that this current flowed through the wires between *mn* or *op* in the opposite

direction to that shown in Fig. 2. In this latter case their effect was to demagnetise the circuit, but now, of course, it must be the reverse. Thus the ampère-turns between *mn* and *op* in Fig. 3 have a forward magnetising effect, and increase the number of lines cut by the active inductors : or, in other words, the armature itself magnetises the field. This result sounds, perhaps, more remarkable than it really is. It must, of course, be remembered that when the brushes are placed at *no*, and the direction of the current is thus the opposite to that shown in Fig. 2 in the direct magnetising turns, any E. M. F. which is generated in these latter is *opposed to the direction of the current flowing in them.* The distribution of the field remains the same as in Fig. 2 ; as, however, the position of the brushes does not now coincide with the neutral line of zero field but is behind it, if the wires between the neutral line and the diameter of commutation are cutting any lines, they generate a *back* E. M. F. It is thus evident that no wire can at one and the same time act as a forward magnetising turn, and also as a useful inductor : the combination of the two functions is, in fact, entirely impossible. The forward magnetising loops, therefore, which Dr. Hopkinson obtained on the armature, in effect were but the translation of some of the usual stationary field-magnet winding on to the rotating armature. The wires themselves, it is true, move with the armature, but their place is continually taken by fresh wires, so that the current-sheets between *mn* or *op* remain stationary in space and magnetise the circuit. The account of the experiment ended with the following pregnant sentence : "If we could put up with the sparking that would ensue, it would be possible to make λ negative in a generator of electricity, and thereby obtain by the re-actions of the armature itself all the results usually obtained by compound winding". This leads us to the method of winding armatures, recently invented by Mr. Sayers (Patent Nos. 16,572, 1891, and 10,298, 1893), and described by him in a paper read before the Institution of Electrical Engineers.[1]

[1] *Journal*, No. 107, vol. xxii., 1893.

Modifying the words of Dr. Hopkinson, we may say that experience has shown that we can *not* put up with the sparking which would ensue if the brushes of an ordinary dynamo were given a backward lead: but in the Sayers dynamo which we are now to consider, the sparking is *annulled* by the particular method of winding the connections between the loops on the armature and the segments of the commutator. Thus even though the brushes be given a negative lead in a generator of electricity, there is a position in which a sparkless collection of the current is possible. This achieved, the prediction of Dr. Hopkinson has found its fulfilment, and a dynamo has been produced in which, by utilisation of the reactions of the armature itself, all the results of compound winding have been obtained.

Let Fig. 4 represent the development on a flat surface of a portion of an armature, the short thick lines such as *bc* being the inductors on the surface of the armature connected together by the slanting portions such as *ab*, which may be either the internal inactive portions of the loops of a ring, or the end connections of a drum winding, in which latter case they will also include a second inductor on the diametrically opposite surface of the core. Each commutator connection, instead of being, as usual, taken straight from the point *a* or *c* to the nearest segment, is turned backwards against the direction of rotation for a short distance along the end of the core, and is then wound over the surface, parallel to and above the ordinary winding: it is then finally joined to the commutator segment in the usual manner as at A or C. The portion of the winding such as *c*C or *a*A is known as a reverser bar, or commutator coil. The brushes are then so placed that the reverser bars which at any moment lead to them are under or near to the trailing pole-tips.

An examination of Fig. 4 will show that of the three portions connected with the brush M, *viz.*, the short-circuited coil *abc* and the two reverser bars *a*A and *c*C, reverser C is moving in a stronger field and is generating a greater E. M. F. than reverser A, while the short-

circuited coil (marked by the open circle) is moving in the weakest field, being farthest from the pole-tip and nearest to the neutral line of zero field. The difference in the E. M. F. of the two reversers which at any time may be under a brush is indicated by the heavy black line of C, and the medium line of A: the other reversers, as shown by the thin lines, are entirely inactive, since they form no part of a closed circuit. Further, when a coil is short-circuited, a circuit *c*-*b*-*a*-A-C-*c* is closed by the contact of the brush surface; hence though the direction of the E. M. F. induced along the length of the two reversers is the same, the one opposes the other, and the greater E. M. F. of C as compared with that of A leaves an unbalanced E. M. F. acting round the closed circuit in the

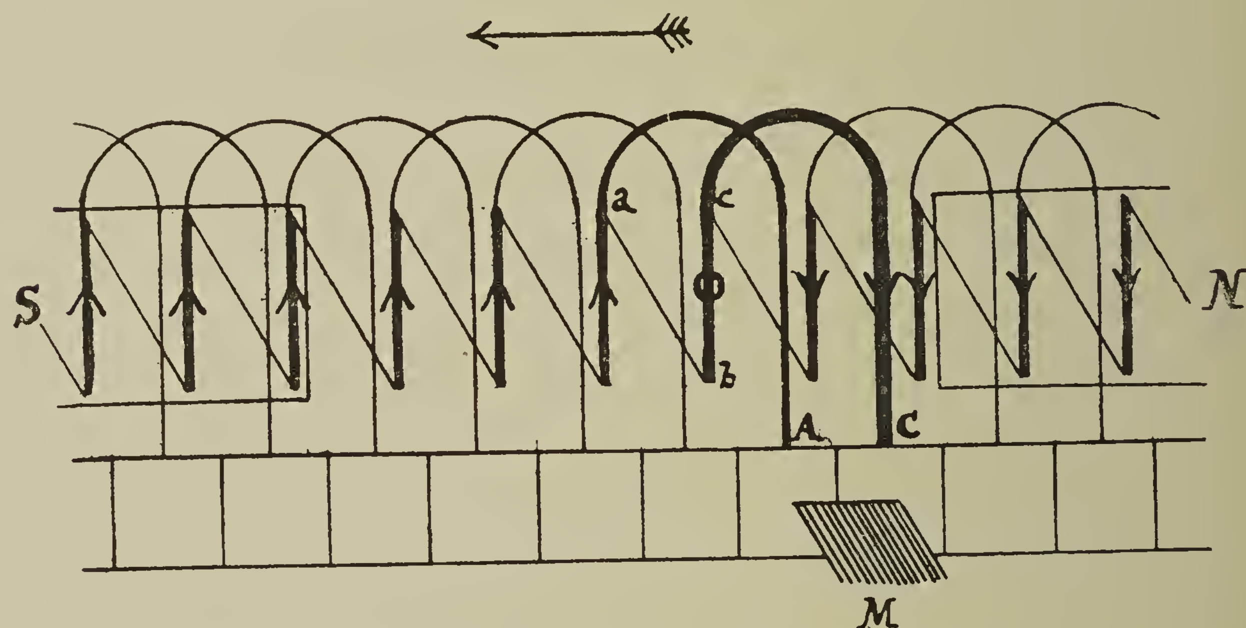


FIG. 4.—Sayers' method of winding Armatures.

direction *c*-C-A-*a*-*b*-*c*. Now the self-induction of section *abc* tends to keep the current flowing in its old direction, *viz.*, from *c* to *a*, this being the direction of the current in the loops to its right, as shown by the arrowheads on the inductors. But it has just been shown that the difference of the E. M. F.'s of C and A tends to drive a current through the short-circuited coil from *a* to *c*. If therefore this unbalanced difference of E. M. F. is sufficiently great, and is given sufficient time, it will stop the old current flowing from *c* to *a*, and will start a reversed current in the required new direction: at the moment when this new current reaches the strength of that which is being carried by the rest of the winding, segment A should pass from

under the brush and the short circuit will be opened without any sparking.

The question then arises, how is sufficient difference of E. M. F. between C and A to be obtained, so that the required reversing action may be provided? The solution of this is to be found in the adoption of a toothed armature core, and a short air-space between the iron of the armature and the iron of the pole-pieces. Such a type of dynamo has other merits which have led to its being frequently tried,—primarily, of course, its great economy in the copper of the field winding, and the electrical energy required for its excitation. But in the long run, experience has so far told against it owing to the difficulty of preventing sparking at the brushes in dynamos of large output. Now, however, the short air-space fits in admirably with the device by which the sparkless reversal of the current is secured, and in fact is essential to it. Let the armature be toothed, and the winding be buried in the slots or tunnels between the teeth, so that a small air-space of, say, one-eighth of an inch may be obtained between the armature and the pole-pieces. Then, owing to the closeness of the iron polar surfaces, the strength of field will fall off quickly as we pass outwards beyond the pole-tips, and the fringe of lines extending into the core from their edges will vary rapidly from a great density to practically zero. If now the winding and position of brushes be as shown in Fig. 4, the coil *cba*, being some distance from the trailing tip of the pole N, will be inducing little or no E. M. F. and will therefore be in a suitable state for commutation. Reverser bar C, being well within the fringe of lines spreading from the trailing pole-tip, will be generating considerable E. M. F., while reverser bar A, being farther away from the pole-tip, will be cutting an appreciably weaker field. Thus by experimentally shifting the brushes, a position may be found for them such that the difference in the E. M. F. produced respectively by C and A is just so great as to reverse the current in *cba* and to bring it up to its normal strength in the new direction, within the time that the coil is short-circuited by the brush.

But now a further advantage of the above method of winding appears. Since the reversing E. M. F. is drawn from the fringe of field at the trailing pole-tip, and this portion of the magnetic field is strengthened by the reaction of the armature current as explained in the preceding article, might not this strengthening be arranged to automatically furnish the stronger E. M. F. which is required to reverse an increased current in the armature? This has in fact been done and Sayers-wound dynamos have been designed and built in which the brushes may be left in the same position from no load to full load without sparking. The attendant is thus entirely relieved from any necessity to shift his brushes as the load on the dynamo varies.

The possibilities of the Sayers-wound dynamo have not as yet been fully explored and delimited, but it certainly holds out great promise for the future, and its invention must be ranked as perhaps the most striking novelty of recent years. The advantages which it possesses may be briefly summed up as follows: the fixed position of the brushes for all loads, the possibility of eliminating armature reaction as one factor by which the output of an armature is limited, and a great saving in the weight of copper and iron required for a given output. This latter advantage is due not only to the conversion of the back ampère-turns of the armature into forward magnetising turns, but also to the fact that a small air-space becomes permissible even in machines delivering large currents. The Sayers method of winding has been applied to a compensator giving eight volts with a current of 400 ampères,[1] in which case there was no field winding at all: and more recently a direct-coupled dynamo of 80 kilowatts output at 420 revolutions per minute[2] has been built, the total weight of which does not exceed three tons. But a still more important case is that of eight large dynamos which have been installed at the chemical works of Mansbo in Sweden[3] for the recovery

[1] *Electrical Engineer*, 14th July, 1893.

[2] *Electrician*, 14th September, 1894.

[3] *Elektrotechnische Zeitschrift*, 6th September, 1894.

of chlorine salts by electrolysis: each of these has an output of 115 volts and 1200 ampères and is a six-pole machine with drum armature coupled directly to the horizontal shaft of a 220 horsepower turbine. In considering the extent to which the Sayers winding is likely to be adopted in the future, perhaps the most serious question which the dynamo designer has to face is that of the cross armature reaction in large machines. The air-space being small, the loops lying under the pole-pieces have a proportionately great cross-magnetising effect: the induction at the trailing pole-tip is thereby forced up to a high figure, and, if care be not exercised in the design, the iron may become saturated either at the trailing pole-tip or in the teeth of the armature core under it, so that the total number of lines is reduced. Hence in the dynamos which have so far been built on the Sayers principle, a double horseshoe field has been usually adopted, which enables each pole-piece to be split in half and an appreciable air-gap to be interposed in the cross-magnetising circuit, without interfering with the true path of the lines into the armature core.

Finally we come to the field of practice, and it is here that the advance of the last few years is most marked. As the use of the continuous current for lighting, transmission of power, traction, and electrolysis has steadily increased, so also have the dynamos for supplying it grown vastly not only in number but also in size. Ten years ago, the dynamos in general use were of 10 to 50 horsepower, but now a unit of 200 horsepower is common in British central electric light stations, while on the Continent 500 horsepower dynamos are frequently met with, and in several cases monsters of even 1000 or more horsepower are successfully employed. Their forerunners are to be found in the Edison "steam dynamo" nicknamed "Jumbo," of which the output was 900 ampères at a pressure of 105 volts, and the "Colossus" Brush machine, for which an output was claimed of 3200 ampères and 80 volts at 405 revolutions per minute. The former is worthy of especial note from its anticipation of modern central-station work in its being directly coupled to the crankshaft of a horizontal steam-

engine at the slow speed of 300 revolutions per minute. The Colossus machine was constructed for reducing aluminium ore in an electric furnace, and the requirement of very large currents for electric smelting by the Cowles or Hérault processes has called forth some of the largest continuous-current dynamos in existence. In 1888 we find the electro-metallurgical works at Lauffen-Neuhausen using a 300 horsepower turbine to drive at the slow speed of 180 revolutions per minute two six-pole Oerlikon dynamos, each supplying 6000 ampères at 20 volts, or 120 kilowatts: each of these dynamos in reality consisted of two armatures wound on the same core, a commutator with six sets of brushes being placed on either side of the machine. In the same year, the Cowles Aluminium Company had at work a two-pole dynamo giving the very large output of 60 volts and 5000 ampères at 380 revolutions per minute. More recently the Oerlikon Company have constructed large multipolar machines also for the smelting of aluminium ores at Neuhausen, in which the armature is mounted on the extended shaft of a turbine and revolves in a horizontal plane within a ring of poles pointing radially inwards. The latest (1893) of these give an output of 7500 ampères at 55 volts or 550 horsepower when running at 150 revolutions per minute: they have twenty-four poles and a huge commutator from which the current is collected by 120 brushes in twenty-four sets; the total weight of turbine and dynamo amounts to as much as twelve tons, and the turbine is arranged to exert an upward thrust so as to take a part of this weight off the footstep of the vertical shaft.

The comparative performance of machines in relation to their size is best estimated by the number of watts per revolution per minute which they respectively give, the speed at which any output is obtained being in fact an all-important item in their comparison. Judged by this test, the output of the Cowles dynamo alluded to above, *viz.*, 440 watts per revolution per minute, must be regarded as very large for a two-pole dynamo. Probably the largest continuous-current two-pole dynamos in existence are those

now in use at the Carnaby Street station of the St. James' and Pall Mall Electric Light Company, and of which mention has already been made as having been exhibited at the Naval Exhibition in London. These give an output of 1500 ampères at 120 volts or 180 kilowatts at a speed of 350 revolutions per minute, and experience has shown them to be most satisfactory, both as regards heating and sparklessness. Their output works out to over 520 watts per revolution per minute, and it is noteworthy that this is attained by the use of a treble-wound armature, *i.e.*, on each armature core there are three separate and distinct windings, their commutator segments occurring in regular succession one after the other, so that every fourth segment belongs to the same winding. Thus the current is collected by two sets of wide brushes resting on the joint commutator, and each set covering not less than three segments simultaneously. It would certainly appear that, by this device of winding the armature so that there are more than the two usual halves in parallel, a larger output may be obtained without trouble from sparking or heating, and that therefore the limit of output within which the two-pole dynamo with its single horseshoe field may be used is extended. Still there must come a point when recourse should be had to a multipolar design, although it is still an open question at what point this limit is reached. On the Continent, as has been already said, multipolar types have found more favour than at home and are more generally used for smaller outputs. In foreign central stations, the dynamos are usually of larger size than with us and are coupled to engines of the slow-speed double-acting class. In a large number of cases they belong to one or other of two types, manufactured respectively by the firms of Siemens & Halske, and Schuckert & Co. The former has internal radial magnets surrounded by a ring armature, this latter being built up of segmental plates and supported on bolts from a hub so as to overhang the magnet system. The Berlin central stations contain the largest examples of this type. At the Markgrafen Strasse station there are four such machines each of 400 horsepower at 80 revolu-

tions per minute, with ten poles and a commutator five feet in diameter. In the dynamos which the same firm has built more recently, a feature of interest is that the separate commutator is discarded, and the numerous sets of brushes bear directly on the peripheral surface of the ring winding; the outer portion of each loop is a copper piece wedge-shaped in section, and is mounted so that the entire number forms in effect a gigantic commutator. At the Spandauer Strasse station there are four engines, each of 1000 horse-power, driving two dynamos which are mounted at either end of the engine crankshaft: each dynamo has ten poles and gives 2000 ampères at 140 volts when running at 60 revolutions per minute, whence each combined set is capable of lighting as many as 7000 lamps of 8 candle-power: while at the Mauer Strasse and Schiffbaudamm stations, there are three and six equally large double steam dynamos. The above gives an idea of the size to which the demand for electric light in towns has raised the continuous-current dynamo. Machines of the same type as above described are also to be found at Copenhagen and Stockholm.

The Schuckert dynamo for central-station work is of an entirely different construction, but again is multipolar. It has a large ring armature of a flat discoidal shape, flanked on either side by numerous poles facing the armature core: opposite poles are of the same sign and in effect form a common polar surface. In recent machines, the armature is adjustable end-ways, so that it may be set exactly central between the poles on either side; all thrust or pull on the core, due to the fields on one side being stronger than those on the other, is thereby obviated. At Hanover there are triple expansion engines of 400 and 600 horsepower, each coupled directly to their dynamos: the armatures, owing to their large diameter of 10 ft. and their high peripheral speed, serve as flywheels: the commutators are of huge size, 6 ft. 6 in. in diameter, and have as many as 840 sections. At Düsseldorf the dynamos have 14 poles and give 400 volts and 1000 ampères at 90 revolutions per minute, and again the armatures are 10 ft. in

diameter with 6 ft. 6 in. commutator. Similar dynamos are in use at the central stations of Altona and Aix-la-Chapelle.

Space does not permit of more than a passing mention of recent progress in traction and transmission of power by continuous currents. For the latter purpose, where the distances are great, the alternating current with single- or double-phase generators and motors, has lately proved a serious rival, but up to the present, for traction, the continuous current reigns supreme, since the distances are usually not so great as to demand a higher pressure than about 500 volts. After the Bessbrook-Newry electric railway and the pioneer lines of Portrush and Blackpool, little was done for many years in England, and the advance was chiefly on the other side of the Atlantic. Once again, however, England has come to the front by the recent construction of the City and South London Electric Railway and the Liverpool Overhead Railway, both of which for size and successful working are, in many respects, unrivalled. In the former, the current is delivered by 225-kilowatt belt-driven dynamos at 500 volts pressure to electric locomotives each containing two 50 horsepower motors. In the latter, each car is furnished with a motor of 40 horsepower, which is built direct on the axle, and the dynamos are driven from horizontal engines by cotton ropes. At the Chicago Exhibition of 1893, the supposed requirements of the future in the direction of rapid municipal traction were exemplified on a large scale by the gigantic generator of 2000 horsepower, which was employed in connection with the Intramural Electric Railway. Nor can this estimate be regarded as much exaggerated, when we find that the West-end tramways of Boston, which form the largest electric system in the United States, are fed by six triple-expansion engines, each of about 1000 indicated horsepower driving by belt eighteen Thomson-Houston four-pole dynamos. Of a different class is the electric rack-railway which ascends Mont Salève near Geneva: here the power station is situated on the Arve, and is furnished with two turbines, the vertical shafts of which drive multipolar Thury dynamos yielding 275 ampères at 600 volts,

or 165 kilowatts, at the very slow speed of 45 revolutions per minute: the dynamos have twelve poles, and thirty-six carbon brushes are used to collect the current. The gradient reaches in places 25 per cent., and each car has two four-pole motors giving 30 horsepower at 600 revolutions per minute, their speed being reduced by double gearing to 50 revolutions of the wheels.[1]

In the transmission of power, properly so called, by continuous currents, much has been done since the pioneer installation by which, in 1886, 50 horsepower was transmitted a distance of five miles from Kriegstetten to Solothurn: in this case the three-wire system was employed with two generators and two motors each of 1000 volts, and the over-all efficiency of the plant was 75 per cent., a result which at that time was regarded as little short of marvellous. It was erected by the Oerlikon Company of Switzerland, and in the succeeding years the same company followed up their first venture by a number of installations of various horsepowers, large and small. Among these may be mentioned the transmission of 60 horsepower over five miles at Lake Lugano, and of 100 horsepower for one mile near Aichberg in the Tyrol with an efficiency of 80 per cent., also with a pressure of 1000 volts. In 1891 they completed the important transmission of 600 horsepower for a distance of 750 yards at Schaffhausen, where the Falls of the Rhine supply the power. Each turbine of 350 horsepower was connected by means of bevel wheels and ropes to a six-pole dynamo with an output of 624 volts and 330 ampères at 200 revolutions per minute. At the spinning mills, to which the power is transmitted, there are two small motors of 60 horsepower each and one large double motor giving 380 horsepower. A commercial efficiency at full load of 78 per cent. was guaranteed by the makers.[2] Finally mention must be made of the very important installation by which as much as 1000 horsepower is obtained from

[1] *Elektrotechnische Zeitschrift*, No. 21, p. 289, 1894.

[2] Kapp's Cantor lectures on *The Electric Transmission of Energy*, delivered before the Society of Arts, 1891.

reservoirs on the mountain river Gorzente and distributed to Genoa and the Polcevera Valley. The fall is divided into three portions, each of which has its own station: the system employed in all alike is the high-tension continuous current with the generators and the motors in series, and they therefore closely resemble one another in their main features. Each turbine of 140 horsepower is directly connected by Raffard insulating couplings to two Thury dynamos and runs at a speed of 475 revolutions per minute. The dynamos are six-pole machines with drum armatures, and are designed to give a constant current of 45 ampères with a maximum pressure of 1100 volts. They are coupled together in series so that the voltage at full load amounts to about 6000 volts per circuit, and owing to this high pressure the insulation of the windings has received great attention. The bedplates of the dynamos are insulated from the ground by means of porcelain insulators filled with rosin oil, and not only are the armature shafts insulated from the turbine by the coupling as mentioned, but, in addition, the actual iron core of the armature is insulated from the shaft itself.[1] From the above it may be seen wherein lies the disadvantage of the continuous current for the transmission of power over very long distances. The nature of the commutator of the dynamo does not permit of more than at the most about 2000 volts being generated in any one machine, and we are therefore compelled to couple several machines in series in order to obtain the high voltage which alone will render the transmission efficient. It is then only by the most careful precautions that the complete insulation of the system can be secured, since the windings of running machines form part of it; whereas with the alternating system it is easy to generate the electrical power at low pressures and subsequently to transform it up to high pressures by means of stationary transformers.

As might have been anticipated, the list of transmissions of power includes but few cases within the British Isles.

[1] *Journal Inst. El. Eng.*, No. 107, vol. xxii., 1893.

Owing to the scarcity of water power, little can be done in this direction at home, and schemes for the transmission of the energy of our coalfields to our towns by electrical stations at the pit's mouth remain but imaginative dreams, far removed from practical realisation.

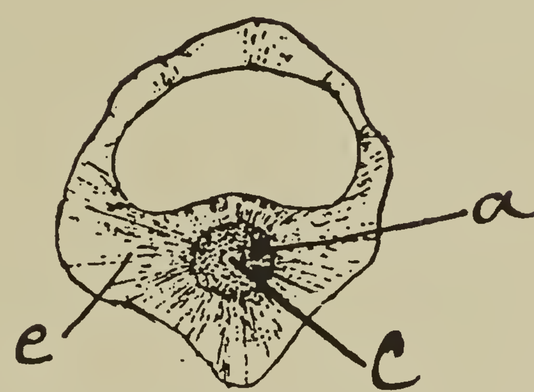
C. C. HAWKINS.

ON THE MORPHOLOGICAL VALUE OF THE ATTRACTION-SPHERE.

PART II.

IN the previous part of this paper I drew attention to the spheres, originally discovered as structures composed of a large mass of protoplasm (archoplasm) containing a clear space and a central body (Fig. I.).

I



If we now take the centrosome in the figure (*c*) as a starting-point, it will be seen that each of the two succeeding zones of the sphere is concentrically disposed to the one internally preceding it. Moreover, this concentric zoning does not stop at the external boundary of the archoplasm (*a*) but is continued beyond it, in the shape of a more or less defined radiation of the general cell contents, which sometimes extends quite out to the periphery (*e*).

Such an external differentiated area of the protoplasm has not, however, generally been considered to be a part of the sphere, probably on account of its extremely fugitive and transitory character ; nevertheless as there is little doubt that the radii (*e*) have sometimes been confounded with the archoplasm, in order to avoid a like confusion, I shall speak of them, when necessary, as the *radial envelope* of the sphere.

Through the researches of Flemming and others we have now abundant evidence of the existence of centrosomes and astral radiations in many kinds of tissue, in as many animal forms. But it becomes at once obvious, on instituting a comparison between their figures, or making preparations for oneself, that the usual appearance of the tissue (somatic) spheres is unlike the three-zoned structure originally described.

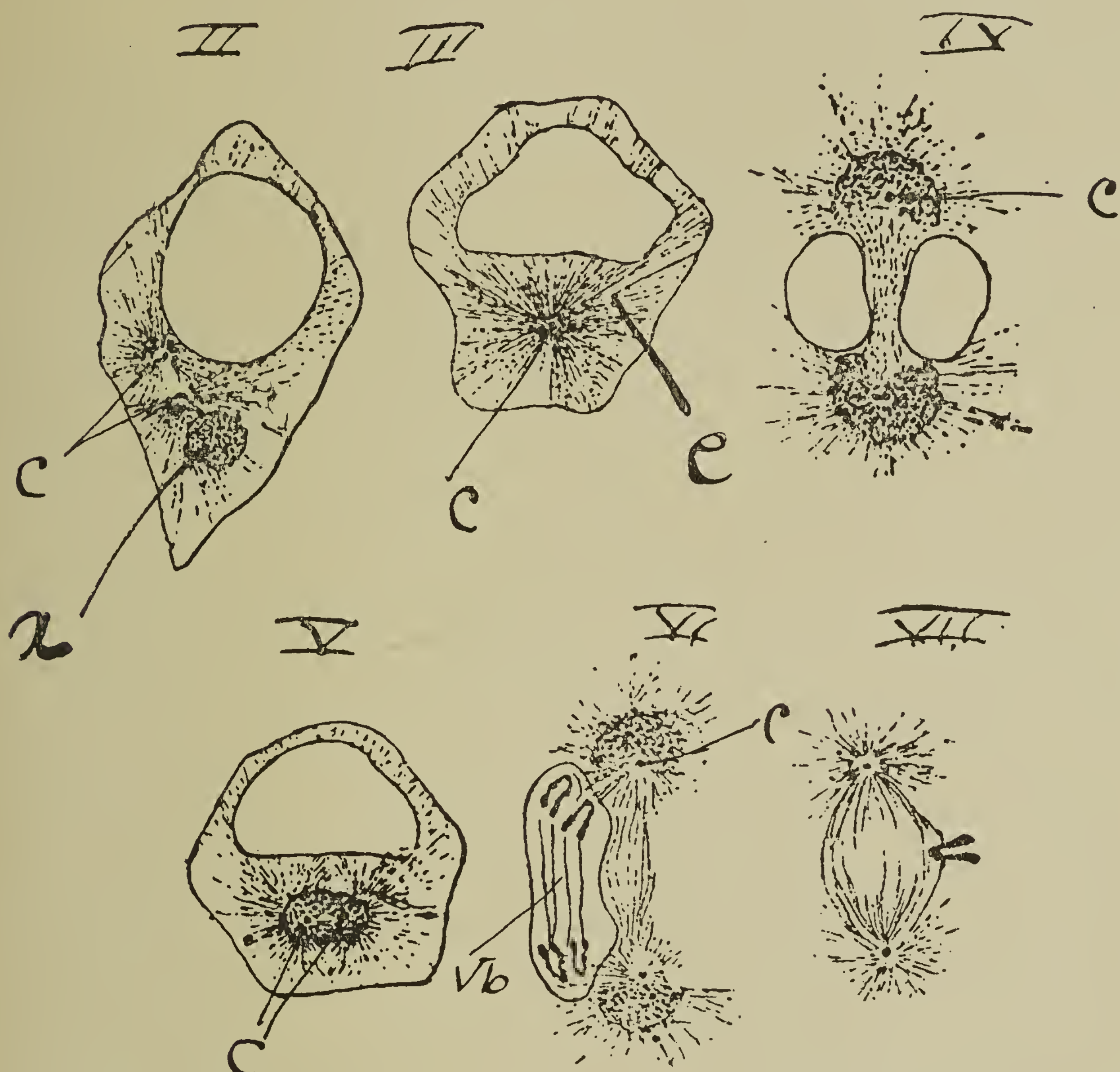
For example, in the cells of the amphibian lung and

mesentery, figured by Flemming, the sphere is composed of a centrosome, enclosed on the outside by the light space (*heller Hof* of the German authors) (Fig. III. *c*), from which spreads a more or less extended radiation through the general protoplasm of the cell. There is, however, nothing comparable to an archoplasmic differentiation of this substance, and in many cases (as for example in some leucocytes) these simple radii extend at once from the centrosomes to the cell periphery. They have in fact exactly the appearance of the radial envelope of a sphere of which the archoplasmic constituent is wanting (*cf.* Figs. I.-III.).

The spheres in these somatic tissues therefore appear to be less complex than those of the reproductive elements with which we have been previously dealing; they may be referred to a *simple* in contradistinction to a more *compound* type, and our appreciation of the mutual relationships of the different modifications in their structure, will, like other problems of comparative morphology, depend upon the possibility of finding intermediate forms.

So far, the compound spheres which we have examined have been such as exist either in ova, or some element of the ovigenetic series. It will be well therefore, for the sake of comparison, to ascertain what is known respecting the nature of these bodies in the analogous spermatic series of reproductive cells. In the spermatocytes of many, in fact I believe of all animals, whether vertebrate or invertebrate, which have been examined with sufficient care, there exists beside the nucleus a large slightly staining body, generally known in the earlier literature as the "Nebenkern". In the case of Salamandra and Proteus, this nebenkern was shown by F. Hermann (18) to contain a central granule, which, at the commencement of mitosis, divides into two halves, that separate from one another through the mass, and draw its substance out between them into an initial spindle figure, since known as *Hermann's Central Spindle*. The subsequent mitotic phenomena occur in relation to these central granules, as if they were true centrosomes, and consequently Hermann was justified in regarding the

nebenkern of the spermatocytes of Salamandra and Proteus as the archoplasmic portion of an attraction-sphere.



Since that time it has been observed that a similar *rôle* is played by the nebenkern in relation to the division of spermatic cells in a great variety of animal forms, and we are now probably justified in regarding the nebenkern as always equivalent to the *archoplasm* of those cells in which that structure was originally described.

When a cell with a compound sphere, like the spermatocyte of a salamander (Fig. V.), is going to divide, the archoplasm becomes so fully used up in the construction of the central spindle, that there is no such residual mass left round the centrosomes as in the mitotic figures of Ascaris (*cf.* Figs. VII., IV.). In this way the external radial envelope becomes directly related to the exposed half of each centrosome, in such a manner as to be quite indistinguishable from the protoplasmic radiations round the centrosomes of a simple sphere (Fig. VII.).

The succeeding events differ somewhat in different cells, as the spindle fibres sometimes, but not always, become equatorially centred to an *intermediate body*, in the division plane of the daughter elements.

Whatever be the exact course pursued, the greater part of the spindle fibres eventually fuse or coalesce to form an archoplasm in each new cell. Into these new archoplasms the centrosomes gradually find their way, and finally come to rest at the centre of each new mass.

Thus, during the course of such divisions, the archoplasm appears to be first modified into an initial spindle figure, and then into a complete one, the residual fibres of which coalesce to form two new archoplasms—one in each daughter cell. In this way the whole sphere has the appearance of persisting through successive cellular generations, archoplasm arising from archoplasm, centrosomes from centrosomes.

If, however, we now turn from the spermatocytes of Salamandra to those of the mammalia, as exhibited in a rat (Fig. II.), the persistence of the archoplasmic constituent in the former, turns out to be no sure or universal guide to its character as a cellular constituent; because, in these mammalian elements, although there is a large and obviously similar nebenkern or archoplasm in the first place, the centrosomes do not, even during rest, come to lie in its interior, but remain quite free in the cytoplasm, generally between the archoplasm and the nucleus (Fig. II. *c*). So it follows that the sphere is here normally dismembered or separated into two portions, one consisting of nothing but the archoplasm, and the other of nothing but the centrosomes. To these latter are centred the protoplasmic radiations, which, in the more ordinary type of sphere, would abut on the outer surface of the archoplasm, and form the exterior radial envelope of the sphere.

In the second place, when division approaches and the centrosomes separate from one another, a spindle is formed, partly from the radiation which originally surrounded these bodies, and probably also with the co-operation of some of the nucleoplasm (Fig. II.). Its whole course of develop-

ment, however, is absolutely independent of the archoplasmic structure, which degenerates, and during the later phases of mitosis vanishes in the cytoplasm altogether. The new archoplasms which arise in the daughter cells, as a coalescence of these cyto- and nucleo-plasmic spindle fibres, are therefore to be regarded as nothing but temporary modifications of the nuclear and cytoplasmic substances, and of an entirely non-persistent nature.

It will be seen that the simple radiation which spreads from the centrosomes in these dismembered spheres, gives to the former bodies exactly the appearance they possess in the tissue (somatic) cells whose spheres are of the simple type; while the concomitant presence of a functionless archoplasm converts the ensemble of cell structures into a form, with spheres intermediate between the simple and the compound types, such as we have been seeking, for it is scarcely possible to doubt that the simple radiation here present round the centrosomes (Fig. II.) is similar to that of simple spheres (Fig. III.). Nor can we resist the further inference that this radiation stands also for what would be an external envelope, were the centrosomes to enter into the archoplasmic substance.

From all this it would appear, then, that the radiation of a simple sphere (Fig. III. *e*) is really equal to the external radial envelope of the compound or reproductive type (Fig. I. *e*), and that in the former the archoplasmic constituent has become (gradually?) suppressed and is now completely wanting. The facts, however, may be recapitulated as follows.

In the first (compound and probably primitive) type of sphere, the archoplasm persists, being alternately stretched out into a spindle figure and then in part recondensed in each succeeding generation. In the second it is not persistent, because, although it arises as a coalescence of the spindle fibres, owing to the extra-archoplasmic position of the centrosomes, it is not reconstructed into a spindle and degenerates in the cell. While in the third or simple type, the spindle fibres do not coalesce to form an archoplasm at all.

Hitherto these differences in the structure of the spheres have been implicitly, if not explicitly, regarded as indications of an indefinite variation in these organs between cell and cell, but if the facts be grouped together (as above) it becomes quite clear that there is some sort of method underlying the apparent meaningless variety of sphere formation, and that the modifications of these structures have probably as real a morphological significance as any grosser features of anatomy. At the same time, however, the fact should not be lost sight of, that, although we may thus split up a cell into zones having different potential destinies with respect to the structures which arise out of them, it does not follow that similar structures always arise from similar cellular zones. In fact this capacity of forming similar things from different sources which cells possess, constitutes an admirable example of the truth contained in Whitman's aphorism, "Organisation precedes cell formation".[1]

It is thus sufficiently apparent, from existing observations on the constitution of the spheres in tissue cells, firstly, that the archoplasm is not always present in them, and secondly, that where this structure does exist it is not necessarily persistent through successive cellular generations. It consequently follows, that the only portion of the sphere which can still lay any claim to persistence, or to a place

[1] The tendency apparent throughout the earlier literature, to regard the nebenkern as a sort of transmissible entity, has probably been the unconscious outcome of the fact that the earlier investigations were almost exclusively confined to cells in which the sphere is compound, and in which consequently there is an archoplasmic spindle formation.

There is perhaps nothing anomalous in the apparent indifference with which the spindle originates either as an intra- or an extra-nuclear structure, for it seems to me merely one mode of expressing what appears to be a fact, *i.e.*, that the kinoplasm (archoplasm) can take up either an intra- or extra-nuclear position. Nor is the dismembered condition of the sphere, to which I have alluded, at all an isolated peculiarity, as Lauterborn's figures of the separated centrosomes and archoplasm in diatoms will show (24). As Ischikawa very justly remarks, with the rather marked exception of the particular animal (*Noctiluca*) with which he deals, R. Hertwig's supposition (25) that the kinoplasm (archoplasm) of the Protozoa is intranuclear while that of the Metazoa is extra-nuclear, appears to have some foundation in fact.

among the prime factors of the cell, is reduced to the narrow limit of the centrosome. "Seine Grosse ist an der Grenze des eben sichtbaren, und bleibt häufig unter dem Durchmesser kleinster Mikro-organismen Zurück," as Hertwig says (26), while these minute constructions are, as we have seen, not in themselves distinguishable from other cellular granulations (microsomes). It is only their position in relation to the rest of the sphere, which confers upon these particles their peculiar distinction, and it is accordingly not surprising that within the last few years the opinion has continually gained ground among observers, that it is possible, if not probable, the centrosomes and microsomes may eventually prove to be really of the self-same nature. This view has found its most complete expression in the theory of Watase, who regards (27) the centrosomes as nothing more than accentuated microsomes, or rather cyto-microsomes as he prefers to call them. He consequently denies them the possession of any properties peculiar to themselves. They only exhibit those which might be acquired by any other cyto-microsomes, their actual peculiarities arising entirely by virtue of position; and, thus considered, they may be looked upon as structures lying in the foci of the forces which divide the cell.

The observations of Heidenhain (8) on the centrosomes in various forms of leucocytes have shown that they may become split up into one, two, three, or four small particles, which in their totality constitute what he terms a *Micro-centrum*, and his investigations of the giant cells of the red marrow show a still further multiplication of these central constituents of the sphere. He maintains, moreover, that the centrosomes grow by a process of continual budding, and that in this fact lies the explanation of the curious and habitual difference in size which they exhibit when paired, either during rest or at the apices of the spindle figure, the smaller being regarded by him as simply the younger of the two. Now, the cytosomes which exhibit this singular capacity for growing and dividing, when subjected to the action of stains, appear by no means all alike. Any one who has actually studied centrosomes will have a lively impres-

sion of the extreme difficulty with which they can be made to take a stain at all, and the frequency with which they remain invisible at the spindle apices, although there may be abundant stained microsomes elsewhere in the cell. It is thus evident that certain microsomes stain when the centrosomes do not, or, in other words, that the micro-chemical behaviour is not the same. When, however, the centrosomes do stain, it is equally true that there are usually seen numerous microsometa from which, but for their position, it would be impossible to distinguish them. Consequently, we are at present incompetent to say whether the centrosomes are what they are, by virtue of more than mere position, or not; because the solution of this difficulty lies beyond our present means of observation, and therefore cannot be determined. The only conclusion warranted by the facts is that the centrosomes do persist through successive generations in many forms of tissue cells.

On turning now to the few cases in which the spheres have already been found to exist in the Protozoa, it will be seen that the main features of the previous description, respecting the modification undergone by the metazoan spheres, are here repeated with a curious exactitude. In Euglypha alveolata, according to the beautiful figures of Schewiakoff (29), centrosomes appear at the apices of the division spindles, which are destitute of archoplasmic surroundings and at the same time intra-nuclear in position. Beyond the unruptured nuclear membrane, fans of radii spread through the protoplasm and are strictly comparable to those of the metazoan radial envelope. Owing to the intra-nuclear position of the centrosomes, the spindle would appear to originate from the nuclear substance, which probably here replaces in part the archoplasm.

In the cystoflagellate *Noctiluca* there is a large extra-nuclear and typically compound sphere, which is comparable in every detail, even down to the little light space surrounding the centrosomes, with the compound spheres of the metazoa. When the centrosomes divide and the central spindle is formed, it lies in a groove in the nuclear membrane, exactly similar to that seen in the embryonic

genital cells of the salamander. The chromosomes are attached to the fibres of the radial envelope, while the Verbindungsfäden (*i.e.*, the fibres in the equatorial region of the spindle after the chromosomes have separated) are produced, according to Ischikawa, by strands of linin becoming stretched out between the separating chromosomes (Fig. VI.).

The sphere is in *Noctiluca* singularly well adapted for the exhibition of the concentric zoning of its structure, even its external envelope being sometimes figured by Ischikawa with a sharp outside boundary, beyond which there stretch the less definite protoplasmic contents of the cystoflagellate's body. I have myself observed in the embryonic genital cells of the salamander a similar sharp boundary to the outer surface of the radial envelope of the sphere, and when we bear in mind that the resting centrosomes, as Heidenhain insists again and again, are to be regarded as the middle point of the whole cell, it is quite possible that the concentric zoning may have some relation to osmotic influences operating on the inner structures from without.

However this may be, the arrangement of the parts of the sphere in diatoms is strangely different from the above. As described by Lauterborn (24) the sphere in these monoplastids is represented at the commencement of mitosis by a small *globe* and a *rod*, which lie outside the nucleus and are quite separate from one another. The globe is the centrosome, and from it stretch radiations comparable to those of the radial envelope of a compound, or the single radial zone of a simple sphere. The rod swells up into a spindle, round which the chromosomes become normally grouped, while the centrosome, dividing, passes to each end of the spindle figure. It will thus be seen that the sphere of a diatom is in the same dismembered condition as the sphere I have described in the spermatocyte of a rat, and that thus the monoplastid analogue of the most curious modification in the polyplastid sphere which I have come across is found among the diatom, one of the most aberrant forms of all unicellular organisms.

Up to the present time, the whole cell has been usually

taken as the morphological unit, but this idea does not apply to morphological conceptions of structures which are themselves intra-cellular in nature. The only units of which we can avail ourselves in such cases are constituted by the microsomes and the more or less definite fluid reticulæ in which they are suspended. In the individual cell, such units are always arranged in an organised complexity, of which the apparent differentiation into nuclear and cyto-plasmic structures is the visible expression. And we have seen further that, beyond this structural differentiation, any superadded complexities of parts are always evolved during the course of cellular ontogeny. Nevertheless, these super-added structures are often purposive, and consequently ought to be regarded as organs of the cell; and it is one of their most remarkable features that such similar and obviously homologous cellular organs are apparently not always evolved from antecedent structures of a similar kind.

We have seen that the spheres in the metazoa are apparently modified in a limited number of ways, and although each type of sphere seems to be restricted to similar kinds of cells in different kinds of animals, concerning the origin or significance of these modifications hardly anything is at present understood.

The ascertained existence of attraction-spheres in relation to the division of the Protozoa is in itself a fact of great importance in comparative morphology, because it banishes once for all any doubt as to the identity of the protozoan body and the metazoan cell; while the curious similarity in the modifications of the proto- and meta-zoan spheres, to which I have herein drawn attention (*supra*), is a fact of which it is at present impossible to estimate the full significance. It is, however, certain that the dismembered condition of the sphere in diatoms, and its correspondence with a similar condition of the sphere in mammals, are facts of more than merely passing interest; and yet they are in no way more remarkable than the correspondence between the other types of proto- and meta-zoan spheres. Few things, one would suppose, could be further apart in the organic series than a *Noctiluca* and the

spermatocyte of a Salamander, yet they both present an identical modification in the parts of their spheres. Can it also be mere coincidence that these organs have been modified along identical lines in things as widely separated as a Euglypha and the spermatocyte of an Ascaris? I conceive rather that these facts must foreshadow the appreciation of morphological generalisations, which will embrace such apparently capricious yet similar variation in the spheres at the extreme ends of the organic series, and that it is among the prime objects of progressive cytology to ascertain what these "laws" may be.

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KEW THERMOMETERS—A CORRECTION.

IN an article on the "Measurement of Temperature" in the September number of "SCIENCE PROGRESS," p. 69, I wrote as follows: "I am afraid that the issuing of those Kew certificates which give the second decimal figure of the correction, so far from being a prevention, is a cause of inaccuracy, as, unless used with a full knowledge of the variations consequent on changes in the conditions, they impart a false confidence to their possessors".

I have received from Mr. Chree information which leads me to the conclusion that I was in error in assuming that the department at Kew issued certificates of the nature indicated. There have passed through my hands altogether three "lists of observations" giving the results of a comparison of certain thermometers with the Kew standard to within ·01°, and I was under the impression that such lists were regarded as certificates; and further, that their acquisition depended solely on the payment of an additional fee.

Mr. Chree points out: (1) that such "lists of observations" are not certificates; (2) that they were only issued under exceptional circumstances to scientific men of well-known standing. He also states that only very rarely, and also under exceptional circumstances, are certificates issued giving the correction to ·05° C.

I regret if my own misapprehension in this matter should have in any way influenced the judgment of others, and I am glad to learn that the authorities at Kew are more cautious than I had supposed.

I cannot, however, refrain from pointing out that their reservations in this matter lend additional weight to the opinions I expressed (in the paper above referred to) as to the difficulty of accurately measuring temperatures by means of mercury thermometers.

Finally, I consider that I have done the department a

service in warning the public against the misapplication of Kew certificates, for instances abound where observations of temperature are given to the $\frac{1}{100}$, nay, the $\frac{1}{1000}$, of a degree by observers who have based their observations on a thermometer standardised at Kew.

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PHYSIOLOGICAL ABSORPTION.

A PLAIN mechanical theory for a process occurring within a living organism is rightly dear to the heart of the modern physiologist. The hypotheses of such a theory are grasped with great clearness by the mind, a clearness enhanced, too, by contrast with the dark background of mysticism formed by the past metaphysical conceptions of Stahl and Whytt. We attempt to explain the phenomena of life on the basis of the knowledge gained by the physicist and chemist, but we essay a hard task ; for if he who deals with inanimate nature must often admit that the conditions under which he works are obscure, how much more is this the case in processes carried out in that tangle of interactions, the living cell!

Can we then feel surprised that the regeneration of physiology that has been so evident since the time of Liebig should, in its over-confidence, have somewhat overstepped the mark, and that in calmer mood we are now becoming convinced that until a clearer insight into the mechanics of the cell has been gained, we cannot hope to grasp the rationale of the simplest processes of the economy?

Physiology is in no real danger of again succumbing to the mysticism of "vitalism," and Rindfleisch's expression, "neo-vitalismus," is certainly unfortunate, but physiology

is recognising that, until the factors of that bald, but as yet necessary, expression "Vital Action" can be analysed and harmonised with physical forces, little progress can be made towards an intimate knowledge of the essence of life.

The history of the development of our ideas concerning the absorption of nutriment is an interesting one, as indicating how ignorant we as yet are of much that is concerned in a process which until recently was thought to be explicable upon purely "physical" grounds.

The classical work of Dutrochet, published at the close of the third decade of the century, was hailed soon after by physiologists as giving an indication of the lines upon which the movements of fluids through membranes in the living animal could be explained. It appeared to be beyond doubt that osmotic action would alone suffice to account for the phenomena of absorption so far as they had then been studied.

Yet, as will be evident from the sequel, we have now to admit that, though osmotic action is a factor, and often of great importance, experiment shows that another factor has to be considered, *viz.*, the living condition of the cells of the membrane through which the solutions pass.

A glance at our present knowledge of the main anatomical points in the structure of the intestinal membrane must precede our physiological considerations.

A multitude of villous processes are clad with specialised columnar cells, whose inner blunt ends rest upon the parenchyma of the organ somewhat condensed at its periphery. A network of blood capillaries is in many places in immediate contact with the ends of these cells, and in the depths of the parenchyma (a more marked structure in carnivors than in herbivors) is imbedded a lacteal tube with blind end and distinct endothelial wall. The epithelial cells are but loosely attached at their inner ends, though cemented together at their sides, and their renewal according to Bizzozero, Heidenhain, Cloetta, and Schaffer is effected from below, from the cells of the crypts of Lieberkuhn, which divide in a plane at right angles to the long axis

of the crypt, and push the new cells upwards. The cells themselves are pliant, easily accommodating their shape to the changes of that of the villus wrought by its specific muscle, and possess at their free border the well-known "basal band" (*basalsaum*) of Kölliker, with the "rodlet organ" (*stäbchenorgan*) of Brettauer and Steinach.

This organ to which so much attention has been devoted is, according to Heidenhain, composed of a number of minute rods in intimate connection with the cell protoplasm and imbedded in a somewhat softer material. The rods can be retracted into the cell, and are also capable of considerable extension beyond it, becoming hair-like, a condition most easily brought about by direct excitation of the surface either chemically or mechanically (*e.g.*, by tape-worms in the gut). As confirmatory of this idea of the rodlet organ must be mentioned the recent work of Miss Greenwood, who in the earth-worm saw appearances suggestive of "retractile cilia" in the gut cells, those cells which are loaded with nutriment having retracted cilia, while those devoid of food stuffs have their cilia extended. Brettauer and Steinach, indeed, thought that the rods were present in the basal band during hunger but absent during digestion.

Thanhoffer, too, has maintained that in the frog, especially when the mucosa is wetted with bile, processes extend from the basal band in the fasting state, and Wiedersheim maintains the existence of a similar state of affairs in the gut of the amphibian *Spelerpes fuscus*. The swallowing of food particles by means of pseudopodial processes in the digestive cells of certain lower Metazoa is known to us through the researches of Metschinkoff upon intracellular digestion, and Wiedersheim admits the possibility of some such action in vertebrates, though the ancestral method is modified, in that by the substitution of extra- for intracellular digestion the cells will only take up matter that has been especially elaborated for them by the digestive ferments. These observations, especially those of Thanhoffer in the frog, urgently need repetition.

The epithelium then of the villus is highly specialised, and, as far as the passage of fluid from the gut to the

blood is concerned, two ways are open, through the protoplasm of the cells themselves, or through the cement substance between these structures. The ubiquitous leucocyte is present in the fluid filling the fine connective tissue sponge work of the body of the villus, and may mount in the soft cement between the surface cells and gain the gut surface. Especially in hunger and hibernation do they pass between the epithelial cells. The oxyphil variety is very frequent, and is most numerous, and contains most granules during digestion. Large conglomerate macrophages also abound, holding the remains of effete cells and micro-organisms.

It will be well at first to confine our attention to the consideration of the absorption of water, and of substances soluble in water which occur in the intestine, *viz.*, salts, sugars, and albuminous bodies.

The older ideas of the absorption of fluids were Imbibition (Rudolphi and Magendie), Osmose (Dutrochet) and Filtration (Brücke), and the merit of first definitely doubting the sufficiency of the two latter processes, to explain the phenomena of absorption in the gut, is due, probably, to Hoppe Seyler.

Hoppe Seyler pointed out that filtration, under peristaltic pressure, and such pressure is never great, cannot be of much value in moving the contents of the gut through its walls, on account of the plasticity of the latter, and, moreover, against the osmotic theory he advanced the fact that weak alcohol in the gut causes no outpouring of water from the blood, but the whole solution is quickly absorbed. He considers that "Bewegungs-vorgänge in den Zellen" are the cause of absorption, and instances the choleraic patient with shed epithelium, whose absorption is at a standstill. It must, however, be stated that Johannes Müller had the idea of the action of the cell long before, for he speaks of it as exerting an "organische Anziehung".

We have many cases in which it has been shown that the diffusion of solutions does not take place with equal ease in living and dead membranes. Claude Bernard found curare passed with greater difficulty across the living than

across the dead gastric mucosa; Susini that the epithelium of the bladder interfered with the diffusion of potassium ferrocyanide to the water in which it was suspended, and Cazeneuve and Livon showed the same thing as regards urea. Fleischer, too, demonstrated that sodic salicylate diffused far more slowly through a living than a dead loop of intestine.

Further than this, it has long been known that diffusive currents may occur with greater ease in one direction than the other through certain animal membranes. As long ago as 1825, Lebküchner showed that solution of potassium ferrocyanide diffused more readily from within outwards than in the reverse direction through the intestine of cat, rabbit, and fox, and Matteucci and Cima found similar differences of osmotic transfer of water across the skins of the frog and eel, and certain gastric mucosa, according to the direction.

Such observations showed that if we are to follow the process of absorption of solutions from the intestine, we must proceed by direct experiment only, and not by deduction from results gained with dead membranes. Experiments have now definitely shown that in addition to the osmotic process, we have also phenomena that cannot be explained except upon the assumption that the living cells themselves exert a "triebkraft" during absorption. We have admitted such an assumption for the act of secretion since the days when Ludwig showed that the secretory pressure exceeds that of the blood, and for absorption, which is practically a reversed secretion, we must now make the same admission.

The data concerning osmose with which we are concerned are as follows:—

1. If the solutions on the two sides of a porous membrane have equal osmotic pressures, such solutions undergo no change of volume.

2. If solutions of unequal osmotic pressure are separated by a porous membrane, water passes from the side where there is a less to that where there is a greater osmotic pressure.

3. The total osmotic pressure of a mixture of dissolved substances is equal to the sum of the partial pressures of the several ingredients.

4. If solutions of equal total osmotic pressures but unequal partial pressures of the dissolved substances are separated by a porous membrane, any constituent of the mixture passes from the side at which it is present at higher partial pressure to the other side, until the partial pressures on the two sides are in equilibrium. No passage of water across the membrane, however, takes place.

If intestinal absorption depends upon osmosis, the facts of experiment must conform with the above laws.

In 1885 a series of researches was commenced in the Breslau laboratory under Heidenhain's direction with a view to deciding the question.

Leubuscher found that solutions at body temperature supplied to a loop of gut in a narcotised dog were absorbed, and better at a pressure of about 10 cm. of water than above or below; unfolding of the intestinal walls obviously at low pressure favours absorption by exposing more surface, while at high pressure compression of the blood-vessels of the mucosa interferes. His most important point was that ·25 to ·5 per cent. sodic chloride solutions were absorbed quicker than water. At 2 per cent. up to 10 per cent. a clear osmotic process was evident, for while the volume of fluid in the gut increased, the amount of sodic chloride fell. With such strong solutions, however, it is, of course, impossible to regard the condition of the epithelium as normal. Comparing the absorption of sodic and potassic chloride, it was found that the former is absorbed faster than the latter, though Graham showed that potash salts are the more diffusible of the two.

Gumilewsky, using a dog with a Vella fistula, confirmed Leubuscher's point regarding the fact that weak sodic chloride solutions are absorbed with greater rapidity than pure water, but added that the relative rates of absorption of water and salt in sodic chloride solutions varies with the concentration.

At a strength of about ·6 per cent., the salt and the

water are taken up *pari passu;* below this strength, the water is absorbed faster than the salt, while above, the salt leaves the gut faster than the water.

Röhmann, also using the dogs with Vella fistula, added some points against the pure osmotic theory. Peptone and sugar, in spite of their well-known differences of diffusibility, he found to be absorbed at about the same pace. Again, in a comparison of the diffusibilities of cane sugar and sodic sulphate, as determined by C. E. Hoffmann, for ox pericardium, with their rapidities of absorption in the living gut, he found that while the rapidity of diffusion of sodic sulphate slightly exceeds that of cane sugar (1·15 : 1), yet the absorption of cane sugar took place at about ten times the pace of that of the soda salt.

These experiments of Heidenhain's pupils had all demonstrated cases which were negative as regards absorption by pure osmose, but no cases of absorption under conditions excluding the possibility of osmose were brought forward. Such absorptive action was, however, shortly shown to be capable of demonstration by Waymouth Reid, who obtained a transfer of normal saline solution across the exsected gut mucosa of the rabbit under conditions of equality of osmotic pressure on the two sides of the membrane, and, moreover, obtained a reversal of the current by adding pilocarpine equally to the two masses of fluid on either side of the intestine.

Heidenhain himself has again quite recently published more experiments emphasising the necessity of considering the process of intestinal absorption as involving other factors besides those of differences of osmotic pressure between the contents of the gut and the blood of the capillaries of the villi. The observations deal with the absorption of solutions of salts, and especially of sodic chloride. For determining the total osmotic pressure of such complex fluids as the serum of the blood, Heidenhain has followed Dreser in choosing the method by estimation of the lowering of freezing point (Raoult), and used the Beckmann apparatus. The method is, perhaps, not so delicate with watery solutions as the blood corpuscle method of Ham-

burger, or even the plasmolytic method of De Vries, but is more applicable to coloured solutions, and is certainly accurate enough for the gross differences dealt with in most of the experiments.

The osmotic pressure of serum is, of course, in the main due to the relatively high partial pressure of the sodic chloride which bulks as the main salt of the ash, for the albuminous substances contribute but a small quota.

That there is such a thing as a physiological act of absorption is again urged by Heidenhain, by showing that if dog's serum of the same osmotic pressure as the serum of the experimental dog is placed in the gut, its water and salts are absorbed in the same ratio as they have in the original fluid, so that the osmotic pressure of the residue in the gut, if the absorption be interrupted, is little different from that existing at the commencement of the experiment; the albumin is absorbed more slowly than the salt, a fact known long ago to C. Voit, but as mentioned above, the albumin contributes but little to the total osmotic pressure of the solution. In one case, serum with $\Delta = \cdot 617$[1] was placed for two hours in the loop of the gut of a dog whose serum had $\Delta = \cdot 626$. At the end of this period, 42 per cent. of the water and 40 per cent. of the salt had been absorbed, and in the fluid remaining in the gut it was found that $\Delta = \cdot 600$.

The same kind of absorption must also be possible for solutions of sodic chloride of the same percentage as the blood; and it will be remembered that Gumilewsky had shown this to be the fact, *i.e.*, that with solutions in the region of ·6 to ·7 per cent. the ratio of salt absorption to water absorption is expressed by unity. Gumilewsky also showed that with solutions of sodic chloride of higher percentage than the blood (since the salt passed into the blood faster than the water), the ratio of salt to water absorption is greater than unity; while, on the other hand, with the solutions of lower percentage than the blood, the water

[1] The symbol Δ is used to express the lowering of the freezing point of a solution, and is proportional to its osmotic pressure.

passed faster than the salt, *i.e.*, the ratio salt absorption to water absorption is less than unity.

This being so, we gather that a "normal" solution of sodic chloride is absorbed unchanged in percentage by a "physiological" action, but that with "weak" or "strong" solutions, the "physical" osmotic action must be superadded.

"In der that weisen schon manche der bisherigen Erfahrungen darauf hin, dass die Vorgänge bei der Resorption sich nicht einfach auf die eine oder die andere Weise deuten lassen, dass vielmehr physiologische und physikalische Factoren in einander greifen, mit je nach den Bedingungen wechselnder Energie, um die Resorption von Salzlösungen herzustellen" (Heidenhain).

Heidenhain, therefore, proceeds to study more minutely the absorption of sodic chloride solutions above and below "normal" strength, upon the assumption that in all cases a "physiological" absorption at "normal" strength occurs, but that the net result is affected in one direction or the other by the concomitant osmotic conditions.

With a solution of sodic chloride in the gut, of higher percentage than the blood (1 to 1·5 per cent. solutions were used), "physically" sodic chloride must pass to the blood, and water must pass into the gut as the total osmotic pressure of the solution in the gut exceeds that of the serum. Since, however, the actual experiments show that the water is absorbed from the gut under the circumstances, the "physiological" output of water exceeds the "physical" income. The absorption, then, of this "strong" solution of salt may be divided into a "physiological" moiety in which salt and water are passing out of the gut, and a "physical" moiety in which salt alone is concerned, for the "physical" water stream is swamped by the opposing stream of "physiological" origin.

Thus the salt absorption here is partly "physical" and partly "physiological," while the water absorption is entirely "physiological".

If the percentage of salt in the "strong" solution be raised to about 2 per cent., the increase in osmotic pressure

may be such that there is, as regards water, a balance between the "physiological" absorbing force and the osmotic pressure. Salt, however, will, under such conditions, continue to diffuse into the blood, and as the osmotic pressure in the tube of the gut is thereby lowered, at a later period, as the incoming osmotic stream of water wanes, it becomes more and more swamped by the opposing "physiological" current.

"So bewirkt die physikalische Diffusion für die physiologische Triebkraft Befreiung von den Fesseln, welche zu hohe endosmotische Spannung des Darminhaltes ihr anlegte, indem sie diese Spannung vermindert."

Turning now to the case of "weak" solutions of sodic chloride (·3 to ·5 per cent.), *i.e.*, solutions with a lower percentage than the blood, we see that "physically" water must pass to the blood with its higher osmotic pressure of salt, and salt should diffuse into the gut; but since here, again, there is a "physiological" output of salt and water, and since the actual experiments show that salt really disappears from the gut, the "physical" salt stream must be swamped by that of "physiological" origin.

In the case, then, of "weak" solutions as contrasted with "strong," the salt absorption is purely "physiological," the water absorption partly "physical" and partly "physiological".

There are then three distinct reasons for the hypothesis of a "physiological" action in the absorption of salt solutions:—

1. Fluids with the same total osmotic pressure as the blood are rapidly absorbed.

2. Water enters the blood from sodic chloride solutions in the gut, whose osmotic pressure exceeds that of the blood.

3. Salt enters the blood from sodic chloride solutions in the gut, whose partial pressure in sodic chloride is less than that in the blood.

The results of diminishing the physiological "triebkraft" will be different in the case of the absorption of the "strong" and the "weak" solutions.

With "strong" solutions, where the water absorption is entirely "physiological," while the salt absorption is partly "physiological" and partly "physical," the net result of interfering with the "physiological" action must be to reduce the absorption of water out of proportion to that of salt.

On the other hand, with "weak" solutions, where the salt absorption is purely "physiological," while the water absorption is partly "physiological" and partly "physical," the effect will be a diminution of salt absorption out of proportion to that of water absorption.

Heidenhain has found in sodic fluoride, added to ·04 to ·05 per cent. of the solutions, a means of poisoning the epithelium, and he finds that when added to the "strong" solutions of sodic chloride, undergoing absorption in a loop of dog's gut, the number representing the ratio of salt absorption to water absorption increases. As we would expect, the absolute salt absorption is found to be also diminished, as well as that of water, but the latter out of all proportion to the former.

The action is recovered from, but only slowly; thus in one case using one and the same loop of gut, it was found that after contrasting the absorption without and with the addition of sodic fluoride, a third experiment without the fluoride showed that it took the loop forty minutes to do the absorption work that before had been accomplished in twenty.

With "weak" solution of sodic chloride the reverse results are obtained; the salt absorption is diminished out of all proportion to the water absorption, so that the ratio salt absorption to water absorption falls, as it should according to the theory. In one experiment the water absorption was not halved, while the salt absorption fell to a seventh of that occurring before the use of the fluoride.

Sodic fluoride, then, is a reagent affecting the water absorption more when "strong" solutions of sodic chloride are undergoing absorption, the salt absorption more when the solutions are "weak," *i.e.*, it affects in each case that part of the total absorption which is to be regarded as of "physiological" rather than "physical" origin.

The result is not due to any raising of the total osmotic pressure of the solution in the gut by the addition of the small amounts of sodic fluoride. The result of raising the total osmotic pressure of a "weak" solution of sodic chloride in the gut by adding, say, sodic sulphate, is that the passage of water out of the gut is diminished, and though by deleterious action on the active-cells the output of sodic chloride might also be affected, the effect upon the water stream must be predominant. Experiment substantiates this, *i.e.*, an exactly opposite result to that got with the fluoride. Moreover, the action of sodic sulphate is only manifest when it is actually present in the gut, while, as seen above, that of fluoride lasts some time after its removal.

Finally, Heidenhain gives the case of two isotonic solutions of different salts placed in the gut. If water is absorbed at all from such solutions, it should, on the purely physical theory, be absorbed at the same rate in each case. But water is absorbed far more quickly from a sodic chloride solution than from an isotonic solution of magnesium sulphate; indeed, a sodic chloride solution whose osmotic pressure exceeds that of the blood loses water quicker than a magnesium sulphate solution whose osmotic pressure is below that of the blood, and the higher "osmotic equivalent" of the magnesium sulphate is not worth reckoning with in such conditions, for the total amount of the salt entering the blood is so small, that its "equivalent" of water entering the gut is but a minute fraction of the large amount of water actually absorbed.

We have referred to this last paper of Professor Heidenhain's at some length, since it represents the results of work that has been going on at Breslau for some years, and the opinion of one highly qualified to treat the subject. It must, however, here be noted that the interference with an artificially set up osmotic stream by the use of a physiological depressant, is not here demonstrated for the first time. The effect was demonstrated, now four years back, by Waymouth Reid, who, in the absorbing skin of the frog, showed not only that chloroform diminished a stream of

normal saline caused to pass osmotically from the outer to the inner surface, but that weak alcohol caused an augmentation.

Some idea of the value of the pressure exerted by the "physiological" action is given by Heidenhain, for if water is absorbed from a sodic chloride solution of higher osmotic pressure than the blood, the pressure against which the absorption occurs can be calculated by the difference between that of the solution and the serum.

In one case, a 1·46 per cent. solution of sodic chloride with $\Delta = \cdot 9$ placed in the gut lost water to the blood with $\Delta = \cdot 6$. The available pressure against the physiological stream which is overcome is represented by $\Delta = \cdot 3$, and this corresponds to a pressure of 2700 mm. of Hg. One can only understand the cells as being capable of withstanding such pressures, by thinking of the extremely minute capillary channels through which the fluids must flow.

The actual rapidity of absorption, though slow, seems to be far quicker in the gut than in an osmometer under somewhat similar conditions. As a maximum, about ·7 cb. mm. of water is absorbed by 1 sq. cm. of absorbing surface (with allowance for the extension of surface produced by the villi) per minute, so that a depth of 7 μ of fluid passes into the cells in a minute. Since a cell is about 35 μ in depth, it takes about five minutes to complete the journey from top to base, a result agreeing well with an experiment of Lehmann's, who detected potassic iodide in a mesenteric vein five minutes after placing the solution in the ileum. An osmometer closed with cow's bladder and filled with defibrinated blood, gives a transfer of only ·023 cb. mm. per sq. cm. per minute, while even with saturated sodic chloride solution in the osmometer, the rate was but ·55 cb. mm. of water per sq. cm. per minute.

As is to be expected, if a physiological act of absorption be admitted, differences of absorptive power should exist in different regions of the alimentary canal, though the circumstances for diffusion are much the same.

Tappeiner showed that in dogs, whilst in the duodenum and jejunum injected taurocholate and glycocholate of soda

were not absorbed, in the ileum both salts rapidly disappeared after their introduction. Again, that watery solution of strychnia was absorbed fast from the intestine but hardly at all from the stomach, though the addition of alcohol at once caused it to be taken up in the latter situation.

Edkins found the absorption of water greatest in the large intestine, hardly occurring at all in the stomach, and taking place at a less rate in ileum than in large gut.

Von Mering, in dogs with duodenal fistula, also found no absorption of water in the stomach (confirmed by Gley and Rondeau), though sugars, in accordance with the older work of Funke, von Becker, Meade Smith, and more recently Albertoni, were absorbed practically in ratio to their concentration.

Lannois and Lépine maintain that glucose is better absorbed in the upper jejunum than in the ileum of the dog, though this is doubted by Röhmann.

Again, Olschanetzky demonstrated the rapidity of absorption of watery solutions in the large gut, by detecting iodine in the saliva of a man five minutes after injection of potassium iodide into the rectum.

The subject of the absorption of watery solutions can hardly be left without a passing reference to the interesting chemical changes wrought by the intestinal cells upon some of the substances undergoing absorption.

Salvioli found that though peptone introduced into a loop of gut with artificial circulation was absorbed, yet it could not be recovered as such in the blood traversing the intestinal wall, and Hofmeister had previously shown that it disappeared somewhere in the mucous membrane. The peptone is probably "regenerated" to albumin, and since the theory of Hofmeister that this function is performed by leucocytes has been disproved by Heidenhain and Shore, we are probably safe in concluding with Neumeister that it is the columnar cells that effect this change during absorption.

Again, since dextrose is the blood sugar, and maltose the form in which sugar is presented to the intestinal epithelium, a change must be wrought during absorption. The

conversion was here originally considered to be brought about by the cells of the Peyers patches (Brown and Heron), but more recent work by Miss Tebb seems to show that the mucous membrane of the intestine is far more active in the process, though it must be admitted that many tissues effect the change.

The interesting syntheses of neutral fats from fatty acids will be referred to below.

It is not my intention in this article to follow the absorption of solutions any further, but it must be stated that the fact that such solutions reach the organism by the blood stream instead of the lacteals is, as Heidenhain has pointed out, easily accounted for by the fact that the capillaries of the villus lie in contact with the absorbing cells. If, however, a great excess of solution is present in the gut, as Ginsberg has pointed out for sugar, and Wertheimer for solutions of indigo carmine, a diffusion within the villus parenchyma may go on into the lacteal, so that the substances appear in the chyle.

If we now consider briefly the question of the absorption of fats, it will be evident that in spite of numerous researches, the actual process is far from being understood.

With regard to the question whether the fat passes through the bodies of the columnar epithelial cells, in the cement between them (Watney), or is transported by wandering cells (Schaefer and Zawarykin), the majority of observers maintain that the first-mentioned channel is the one normally used. Fat certainly appears between the columnar cells in microscopic preparations at times, but it is probably forced there from the villus parenchyma, during the contraction of the villus muscle that occurs when the tissue is plunged into the fixative, for fat is not so found between the cells of the non-muscular processes of the frog's gut (Heidenhain). Fat granules in leucocytes also undoubtedly occur, but Zawarykin's idea of the great prevalence of fatty leucocytes during digestion is, according to Heidenhain, an error of observation due to mistaking the granules of the oxyphil leucocytes for fat on account of their black reaction with osmic acid.

If it be decided that the fat is taken up by the columnar cells, we ask, in what form is it absorbed, and by what means do the cells accomplish their end?

There are two main theories: the one, that the fat is absorbed in corpuscular form as minute globules of an emulsion; the other, that it is in some way first got into solution, and the solution absorbed, the fat being subsequently again separated from the solution.

It has been advanced against the corpuscular theory, which appears to be the more prevalent of the two, that the epithelial cells refuse to take up minute particles offered to them.

Donders could get no evidence of "choroidal" pigment grains in the cells after feeding the animals on eyes, and Grünhagen could not succeed in making the cells lining the frog's gut take up particles of carmine. Tomasini has, however, lately maintained that starch granules, as such, can be taken up and recognised *in situ* by iodine. Then, again, in the microscopic preparations the fat particles are not seen in the basal band, or even immediately beneath it as a rule, but first appear at some distance within the protoplasm of the cell. The emulsion, too, of fat within the digesting gut is said by Cash to be not fine enough for the purpose, and Krehl, who fixed the whole gut with its contents in the case of *Triton* fed with cream, while he saw fine fat grains in the cells, only found conglomerate masses in the lumen of the gut. Certainly the fat does not attain the fine state of division which we know as the "molecular basis" of the chyle until it has actually passed the wall of the central lacteal, and unless we imagine a special "selective power" for the cylinder cells, it is difficult to explain why they should refuse a carmine particle but take a fat grain. Even the exsected gut of the frog, as Grünhagen has shown, will take up olive oil, especially when it is also supplied with the animal's bile, but refuses carmine particles.

The theory of the absorption of fat in solution was originated by Perewoznikoff, and shortly after developed by Will. The idea is that from the fatty acids set free by the action of the pancreatic ferment, soaps are formed with the

alkali of the bile and pancreatic juice, the soaps and glycerine absorbed, the fatty acid again set free in the cell (the alkali perhaps being used again), and recombined with the glycerine to form neutral fat.

Will fed frogs on pillules of pure palmitic acid, the melting point of which is, of course, far above the body temperature of the animal, and found distinct fat globules in the cells of the gut. He states that he controlled the results with observations on the appearances in the gut of starved frogs; a necessary precaution, since it has been shown by Grünhagen that the gut cells of starving winter frogs are loaded with fine fat grains, though the appearance is a very different one to that seen in a frog fed with fat. Ewald, too, kept exsected gut mucosa at body temperature in a solution containing soap and glycerine, and also maintains that he got evidence of fat grains in the cells.

Altmann has suggested a solution of the free fatty acids in the bile salts, a fact demonstrated to be possible by Strecker and Latschnikoff, and naturally maintains that his "granula" are the active agents in the regeneration that follows absorption.

Now, we know from the work of Immanuel Munk not only that considerable amounts of fatty acid can be detected in the gut of a dog at any time during the digestion of a meal of the neutral glyceride, but also that animals may be nourished upon fatty acids as well as on fats, provided an equivalent amount is supplied; and further, that the fatty acids given by the mouth appear in the chyle to a large extent, though not exclusively, in the form of neutral fats.

A few years back, a lucky case of chylous fistula in a patient enabled Munk to demonstrate upon man the truth of these assertions. In this case, practically the whole chyle flowed at intervals from the fistula. Olive oil by the mouth was collected as such at the fistula, mutton fat gave a chyle fat solid at the temperature of the room; but erucic acid from rape-seed oil gave the neutral fat erucin.

But the most elegant experiment was with spermaceti. This has a melting point of 53° c., and is a compound of palmitic acid and cetyl-alcohol; 14 per cent. of the weight

of the spermaceti eaten was collected at the fistula, but the material collected melted at 36° c., and was not spermaceti but ordinary tri-palmitin. Thus, the spermaceti was split, and the free fatty acid finding glycerine, from what source we know not, appeared as the neutral glyceride, the cetyl-alcohol not being detected.

Again, the fat of the chyle is known not to be capable of accounting for all the fat absorbed from the gut, so that even the supporters of the corpuscular theory must admit either that some soluble material is absorbed by the blood path, or that the deficit is metabolised *in situ*. Frank ligatured the thoracic duct, and still found that some portion of the fatty acids with which the animal was fed was being absorbed, but his blood analysis gave no help; further, he found the rapidity of output of fat at the thoracic duct on feeding with fatty acids was far behind the rapidity of absorption from the gut, and this difference was more marked than when the animal was fed with neutral fats.

Those in favour of the corpuscular theory may advance the undoubted fact that there is a distinct relation between the melting point of a fat and its economical absorption, a fat fluid at body temperature being far better utilised than one that is solid. Yet Munk and Arnschinck show clearly that a certain amount of fat of higher melting point than the body temperature is certainly absorbed. Arnschinck, for instance, found that from 9 to 14 per cent. of stearin melting at 60° c., and which is fairly hard even at 40° c., was absorbed. To what extent the bile is necessary is not very clear. V. Wistinghausen's theory of its action is hardly needed if we deny the presence of capillary tubules in the "basal band" of the cylinder cells. Dastre has certainly shown that if a dog is fed on a fatty emulsion such as milk, it can absorb fat without bile, and gain in weight; and Munk also had a dog with biliary fistula absorbing 100 grammes of lard a day, though with fats of higher melting point he admits the necessity of the presence of bile, for while 90 per cent. of mutton fat is absorbed by a normal dog, only 36 per cent. is taken up without bile.

In favour, perhaps, of the theory that fats must be split before absorbed, is the fact that Nencki found the absence of bile lessened the splitting power of the pancreatic juice, and Munk finds that neutral fats are not so well absorbed (by 6 per cent.) as free fatty acids by dogs with biliary fistula.

In fine, since the only histological observation of the actual corpuscular theory of absorption of fat by the cells is that of Thanhoffer, an observation which, we repeat, needs confirmation, we must at present reserve judgment as to whether the fats enter the cells in some form of solution, or are actually "swallowed" in fine particles. The droplets, once in the cells, are forwarded to their deeper ends, and then discharged into the fluid filling the meshes of the villus parenchyma. From here, probably, by the action of the pressure produced by the villus-muscle, they are forced in suspension through the wall of the lacteal, and this possibly causes a finer division, for the "fett-staub" of the chyle is composed of far finer particles than those seen within the sponge of the villus.

We can only understand the progress of the fat particles within the cells as being produced by some kind of motion analogous to "streaming movements" of protoplasm, but this has not been definitely observed, unless perhaps by Spina, in the cells lining the gut of the common house fly, and when we come face to face with the ultimate cause of that "physiological" absorptive action for fluids, spoken of above, we can only imagine something of the same kind.

Many have admitted active changes of shape of the cylinder cells, but Spina's observations form the only instance in which it has been definitely maintained that a regular periodic change of shape is visible during an act of absorption.

The gut cells of fly maggots, especially when fed upon frog's muscle stained with methyl violet, Spina maintains, can be seen to swell at their free ends, suck up the violet solution, and then contract and pass the solution towards the body cavity. He says that these cells refuse to take up the coloured solution from the body cavity and "secrete" it into

the gut; the cells only load from the gut, and discharge into the body cavity. He further maintains such movements are visible in the cells of the absorbing skin of the frog, when the edge of the web is observed in profile.

Heidenhain is doubtful of these observations, on account of the ease with which passive changes in the shape of the cells produced by contraction of the musculature of the gut may be mistaken for those possibly of active origin.

Whatever may be the final decision as to the mode of production of "physiological" secretory and absorptive pressure, it must be admitted that the time has now arrived when the latter must be as definitely accepted as has been the case with the former for more than forty years.

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E. WAYMOUTH REID.

COAL: ITS STRUCTURE AND FORMATION.

PART I.

"AMONG the humiliating proofs of our limited powers of inquiry, there are few which are more striking than that which is manifested by the inefficiency of our investigations relative to coal."[1] With these words, eighty-three years ago, Parkinson began one of his letters "on the opinions respecting the formation of coal". Since his day our knowledge of the Coal-Measures has gradually increased, but we cannot yet afford to regard his words as entirely inapplicable to our present position. In another letter, the same writer thus sums up his own views as to the manner of coal formation: "The opinion, which the strictest examination of every circumstance seems best to warrant the adoption of, appears to be, that coal is a product of the vegetable matter which has been buried at several distant periods, but chiefly in consequence of a universal deluge; and which, after having been reduced to a fluid state by the bituminous fermentation, has suffered a certain modification of that inflammability which bitumens in general possess, by the deposition of its carbon, and by an intimate and peculiar intermixture with various earthy and metallic salts".[2]

It would take us far beyond the limits of a single article to make any serious attempt to follow the gradual growth of our knowledge of coal, or the development of the theories which have, from time to time, been propounded as to its mode of formation. Some of the earlier theories of coal formation, such as we find in the works of Sternberg[3] and Link,[4] regard the seams of coal as the altered accumulation of masses of vegetable matter deposited as water-borne sediment. The latter writer, whose pages contain many

[1] Parkinson, vol. i., p. 233.

[2] *Ibid.*, p. 248.

[3] Sternberg, Fasc. i. and ii.

[4] Link, p. 43.

suggestive expressions of opinion, far in advance of the general thought of his day, refers to the occurrence of upright stems in the Coal-Measures, and realises the possibility that a vertical position may sometimes be the result of the action of water, and that it is not necessarily a proof of growth on the spot.[1] Within the last few years the elaborate memoirs by French geologists on the coalfields of Central and Northern France, have clearly demonstrated that in certain cases the true explanation of coal formation must be sought in the direction suggested by some writers in the early part of the century. The remarks by Link with reference to erect stems are especially interesting in view of the opinions recently expressed by Fayol[2] on the same subject.

We may approach the subject of coal from various points of view, and any one of these suggests diverse lines of inquiry which have not as yet been thoroughly exhausted.

We may turn our attention to the numerous questions of special geological interest which centre round the nature and mode of origin of the coal seams; we may study the composition of coal in the laboratory with a view to solving the problems connected with the conversion of vegetable *débris* into a hard and compact rock; we may, on the other hand, take up the investigation of the coal strata from the point of view of botanists, anxious to learn something from the fragmentary remains in the Palæozoic herbaria as to the lines of descent of existing plants; and, finally, we may make more or less feeble attempts to picture to ourselves the actual geographical and climatal conditions which obtained during the building up of the great series of strata included in the Coal-Measures. The immediate purpose of this article is to draw special attention to some comparatively recent researches into the conditions of coal formation. The usual explanation of the manner of accumulation of coal seams, which we are accustomed to in English text-books, has

[1] Link, p. 44.

[2] The works of the various authors will be found in alphabetical order in the bibliography accompanying this article.

gradually come to be distrusted as inadequate to explain certain facts.

In 1842 Logan drew the attention of geologists to the constant occurrence in the South Wales coalfield, and in other districts, of a characteristic unstratified argillaceous rock underlying each bed of coal. It was shown by this observer that the underclay not only occupied this constant position, but that it was further characterised by containing numerous stigmarian remains. The idea of an old surface soil was gradually accepted as the most satisfactory interpretation of these facts. It was held that the beds of coal had been produced from a thick mass of vegetable *débris*, which had accumulated during centuries of forest growth on the underclay surface soils ; and in the stigmarias were recognised the forked roots of sigillarian stems which had largely contributed to the substance of the overlying coal. Bowman and others gave strong support to this view, and the former, in his theory of intermittent and irregular subsidence, found a convenient means of explaining certain peculiar features in the arrangement and relative positions of seams of coal.

During the last fifty years there have been numerous writers who have warmly advocated the theory that the coal-forming materials accumulated on the surface of forest-covered areas, and that, after subsidence had brought about a general submergence, the vegetable remains became sealed up under a covering of mud and sand.[1] Leaving out of account any differences in detail, the general concession of opinion has, until recently, been strongly in favour of this so-called growth-in-place theory of the formation of coal. It has come to be regarded as the orthodox standpoint from which to explain most conveniently the facts of Upper Carboniferous stratigraphy. We may briefly summarise some of the main arguments quoted in support of the growth-in-place views : (1) The almost constant occur-

[1] An interesting account of coal building on drift theory lines will be found in *Coal* (see bibliography). Hull devotes a chapter of his book on the British coalfields to this subject.

rence of the underclay and its stigmarian fossils under every bed of coal; (2) the remarkable absence of arenaceous or argillaceous impurities, and the uniformity of some coals in composition and thickness over a wide tract of country; (3) the not uncommon occurrence of upright stems of trees in strata associated with the beds of coal. Various authors have successively passed on these duly accredited arguments, without always pausing to think whether or not they form a fatal objection to some other mode of origin than the carbonisation *in situ* of a semi-decayed mass of forest *débris*. A glance at a series of chemical analyses [1] of anthracite, coal, lignite, peat and wood, shows a gradual increase in the percentage of carbon, and a corresponding decrease in other elements. From these and other classes of facts, it has been argued that we have in coal and anthracite the extreme terms of a fairly continuous series of vegetable deposits, which, speaking generally, are richer in carbon, according as they belong to older rocks, and have been longer exposed to slowly acting chemical changes. A connection between an increase in carbon percentage and the amount of earth movement to which the strata have been exposed, lends support to such opinions.

The unusual character of Carboniferous lignitic deposits in Central Russia, made up of paper-like laminæ of little altered cortical tissues,[2] has been attributed to the escape of the beds from the effects of earth movements, and from the influence of those potent factors, heat and pressure, which, in other cases, have accelerated and extended the chemical changes to a much more advanced stage in the process of carbonisation. Among Tertiary rocks we occasionally find carbonaceous deposits which would be placed in the category of ordinary coal, if they were not members of a much more recent geological system. Pressure and heat may have played important parts in the production of coal; but the series of changes involved in the alteration of plant tissues into compact coal, have been far

[1] Toula, p. 22, and in many other works on coal.

[2] Figures of these tissues are given by Zeiller in a paper published in the *Ann. Sci. Nat.* (Bot.), No. 6, vol. xiii., p. 213, 1882.

too complex to allow us to assign definite reasons for the present form of carbonaceous deposits. The element of time is constantly referred to as one of the guiding factors in the formation of lignitic deposits and true coals, but granting its importance in geological changes, it has, in all probability, been drawn upon too freely as a means of accounting for certain phenomena. The evidence of recent research seems to point very distinctly to a much more rapid formation of coal than has previously been supposed. It would seem that we have no good grounds for asserting that modern peat formations, or certain Mesozoic and Tertiary lignites, would ever assume the characters of true coals, however much time be allowed for future changes. There has been at certain times in the earth's history a concurrence of special conditions, which have rendered possible the formation of coal deposits on a large scale, and these conditions were especially characteristic of Upper Carboniferous times. We are certainly not justified in adopting Lesquereux' dictum[1] that peat bogs are nothing but beds of coal "not entirely ripe or burned out". The central idea of the growth-in-place theory may be summed up in a sentence from Geikie's text-book of geology : " Each coal seam represents the accumulated growth of a period which was limited either by exhaustion of soil, or by the rate of the intermittent subsidence that affected the whole area of coal-growth ".[2]

Following the early views of Sternberg and others, several writers have in recent years advocated in some form or other the formation of coal strata by the drifting of vegetable *débris*, and its subsequent deposition on the floor of a lagoon or sea, with an accompanying series of arenaceous and argillaceous sediments.

Theories of this class which do not regard coal seams as old peat bogs, or as the remains of forests which grew on the underclay soil, are usually referred to collectively as the drift theory of coal formation.

For these opposing sets of views Gümbel[3] has instituted two new terms which have been adopted by a few writers ;

[1] Lesquereux, p. 842. [2] Geikie, p. 808. [3] Gümbel, p. 201.

the growth-in-place method of formation he speaks of as the autochthonous origin of coal, and the building up of coal from drifted material is designated the allochthonous mode of origin.

Among the older views as to the nature and origin of coal, we occasionally meet with the suggestion that the greater part of the carbonaceous substance has been derived from bituminous eruptions. It has been generally agreed that coal is almost entirely made up of carbonised plant fragments, and the idea of any extraneous source of carbon has been allowed to drop. Recently, however, this old theory has been revived, and some new arguments set forward in its support. Before passing on to consider the allochthonous mode of formation, we must take note of this third theory which M. Rigaud has seriously discussed in a recent number of the *Revue Scientifique*.[1] This writer's main contention is that plants have played a subordinate *rôle* in the formation of coal, and are by no means mainly responsible for its production.

Coal usually exhibits traces of plant tissues embedded in a black substance, and this homogeneous matrix may be regarded either as a bituminous substance of volcanic origin or as a product of vegetable decomposition. Assuming a tropical climate for Coal-Measure times, Rigaud points out the unsuitable nature of tropical plants, and the still more unfavourable climatal conditions for the formation of anything of the nature of peat.

Neumayr and many other writers have drawn attention to the absence of peat in tropical countries, and have used this fact either as an argument against a tropical climate during the coal period, or as an obstacle to the growth-in-place method of accumulation.

Rigaud lays stress on the absence of certain elements in the ash of coals, which ought to be present, on the assumption that the carbon has been derived from plant tissues. If coal consists of altered vegetable *débris*, we ought to find a certain amount of alkalies and phosphoric acid in its ash ;

[1] I am indebted to Prof. Zeiller for calling my attention to this article.

the absence or very small quantities of these substances is, he contends, a serious argument against the generally accepted vegetable nature of coals. Had such substances ever been present, it is difficult to understand how they could well have been removed by the solvent action of water ; the amount of maceration in running water necessary to eliminate these ash constituents, would probably involve the destruction of all traces of organic structures. It is always easy to fall back upon vaguely expressed chemical changes as a convenient means of explaining certain facts, but the point urged by Rigaud is one which should receive attention at the hands of those qualified to deal with this branch of the subject. The opaque black substance occasionally met with in the cavities of coal tissue cells, is regarded by this writer as so much bituminous material which has forced its way into the empty spaces. We have abundant evidence, he suggests, as to the eruption of hydrocarbons in past time in the bituminous shales, asphalte, and other similar substances. If such outpourings took place on a large scale on the floor of a lagoon or gulf into which water carried down vegetable and mineral sediments, the conditions would be favourable for the formation of beds of coal, and somewhat analogous to those which at present obtain in the pitch lake of Trinidad. The author of this latest theory of bituminous eruptions, claims for it that a critical examination of the facts and arguments should result in the verdict that it leads us a little nearer to the truth than the previously accepted explanations of coal formation.

Any attempt to explain the manner in which coal has been formed, must to a certain extent be founded on the facts of microscopic structure. The occurrence of distinctly marked impressions of plants on the surface of a piece of coal is fairly common, and in the absence of any definite markings imprinted on the surface, we can frequently detect a fibrous structure in the dull layers of mineral charcoal or mother of coal. In some places masses of coal are obviously made up of flattened pieces of *Sigillaria*, *Lepidodendron*, and other plants. Such instances are figured in Goeppert's famous dissertation on the structure and formation of coal.

In some French coals there are distinct signs that the rock is little more than a thoroughly carbonised mass of strap-shaped *Cordaites* leaves. A careful naked-eye inspection of some coals reveals the existence of small compressed circular or elliptical bodies which, on isolation and microscopical examination, are found to show a very striking resemblance to the macrospores of some recent pteridophytes. The marked similarity of the fossil and recent forms has been demonstrated by Williamson and other writers; it is very clearly shown in some figures given by Kidston and Bennie in a paper dealing with Carboniferous macrospores.

The microscopic investigation of coal is necessarily attended with some difficulty owing to the opaque nature of the material. Some observers have adopted a method of examination by means of specially prepared semi-transparent sections; others prefer to treat pieces of coal with potassium chlorate, nitric acid, and other reagents, in order to isolate the plant tissues. Suggestions as to manipulation and the preparation of sections will be found in the contributions of Gümbel, Dawson and others.

The great pioneer work of Witham of Lartington on *The Internal Structure of Fossil Vegetables*, includes a short description and a few figures of microscopic preparations of cannel and other coals. Shortly after the publication of Witham's results, Hutton recorded the occurrence of some light wine-yellow coloured material in the cavities of plant cells, as seen in thin sections of certain kinds of coal; this was regarded as apparently a bituminous substance, which he found to be readily expelled on heating. These observations of Hutton are of special interest in connection with some startling results recently published by MM. Renault and Bertrand on the boghead coals of Central France, Scotland, and Australia. In 1838 Link gave some account of the microscopic structure of peat, lignite and coal; he figures various fragments of plant tissues, and small resinous orange-coloured bodies, the nature of which he leaves undecided. He regards coal as the peat of a former geological age. In 1857 Bennett made a detailed examination of the structure

of the Scotch boghead or torbanite, a substance rendered famous by the legal proceedings in the case of Gillespie *v.* Russel,[1] and by the scientific evidence of experts in favour of and against its close connection with coal. Bennett figures some sections of ordinary coal showing crushed resinous spores, also sections of the boghead containing numerous transparent bodies of irregular outline, embedded in a black matrix. The bituminous boghead is considered to be essentially distinct from coal in the absence of obvious plant structures. Bertrand and Renault,[2] who have lately examined the Scotch boghead, give an entirely different interpretation to the yellow bituminous bodies of Bennett and others; they look upon them as the well-preserved remains of fresh-water algæ.

Balfour dissents from Bennett's views as to the nature of boghead, and prefers to class it with true coals; his paper on this subject contains a figure of an interesting example of "spore" coal.

Without attempting to give any complete account of the earlier contributions to our knowledge of coal structure, such as those by Quekett, Phillips, and others, a brief reference may be made to Huxley's description of what he regarded as sporangia and spores in the well-known Better bed coal of Bradford. Williamson[3] pointed out the fact that the so-called sporangia were macrospores, and the smaller bodies microspores; this palæobotanist gave a short preliminary account at the York meeting of the British Association in 1881 of his researches on the structure and physical composition of coal. For several years the task, which Professor Williamson set himself, of making a "systematic series of microscopical observations on the coals of the entire world," has been gradually proceeded with; and the publication of these researches should put us in possession of many important data, from which we may expect to obtain further light on the question of coal formation.

[1] An account of the evidence given at this trial by scientific experts will be found in Quekett's paper.

[2] See references given in Bertrand's paper, also "SCIENCE PROGRESS," vol. i., p. 60.

[3] Williamson (2).

Dawson adopted the method of chemical treatment in his investigations on the minute structure of coal, and has been able to identify numerous fragments of scalariform and pitted vessels, and other tissue elements; he was led to the conclusion that the existence of spore-bearing beds is an exceptional rather than a common occurrence. In speaking of the nature and origin of coal, Dawson sums up the question as follows: "In short, a single trunk of *Sigillaria* in an erect forest presents an epitome of a coal seam. Its roots represent the stigmaria underclay; its bark the compact coal; its woody axis the mineral charcoal; its fallen leaves, with remains of herbaceous plants growing in its shade, mixed with a little earthy matter, the layers of coarse coal."[1]

Some further information as to the occurrence of spores in coal is contained in a contribution by E. T. Newton, in which special attention is directed to the numerous spores in the Australian white coal or Tasmanite; the name *Tasmanite punctatus* was suggested for these spores, but it has not been generally adopted, and, indeed, such a designation does not seem particularly appropriate.

We are indebted to another English geologist, Wethered, for additional facts as to the various spore coals; he has figured and described several forms of macrospores and microspores in the Better bed and other seams. He points out the abundance of spores in the dull layers of certain kinds of coal, and suggests the rather unfortunate term, hydrocarbon, for the structureless substance which chiefly constitutes the bright patches in coal seams.

Brief reference must be made to the work of Reinsch, whose patient investigations resulted in the discovery of numerous structures in coal which he was at a loss how to dispose of in any existing classification of plants. He finally decided to consider them as plants of a specially primitive type; some he compared to Myxomycetes, and others were provided with special polysyllabic designations, and consigned to classes or groups instituted for their reception. In some of these structures he recognised a resemblance to sphærocrystals, but, unfortunately, the temptation

[1] Dawson (1), p. 638.

to regard them as unknown forms of plant life proved too strong, and they were all laboriously figured and defined as new types of protophytic genera. The majority of Reinsch's genera must be regarded as inorganic structures, whilst others are clearly founded on plant tissues or spores. In addition to various fragments of vascular tissue he recognised several spores, and to these applied the generic term, *Triletes*, which some authors have found convenient. Following these investigations we have a paper by Fischer and Rüst, in which Reinsch's conclusions are called in question, and the various strange "plants" are regarded as examples of the numerous forms assumed by resinous and other substances which enter into the composition of coal. The same observers mention the occurrence of several definite crystalline substances in certain forms of coal, *e.g.*, Fichtelite, Hartite, etc. A detailed account of such bodies in coal rocks, and of the various forms of coal, lignite, etc., has lately appeared in the third volume of Zirkel's new edition of his *Lehrbuch der Petrographie*. In 1883 Gümbel published a valuable account of the minute structure of coal, lignite, and peat; his method of examination is fully described, and the results obtained bear out the advantage of a chemical treatment in certain cases. He points out that Reinsch included in his protophytic genera such structures as dendritic crystals of sulphur, and various other mineral substances. The same author treats of the method of conversion of plant tissues into coal, and disputes the accuracy of the common statement that anthracite is simply coal which has been subjected to greater pressure. The occurrence of anthracite not merely in the deeper layers of a coal series, but between or above ordinary beds of coal, suggests some other factor than increased pressure and metamorphism. In discussing the question of coal formation, Gümbel recognises that there must have been different methods by which the same results were obtained; but, on the whole, he is disposed to agree that there are good grounds for the comparison of Palæozoic coal seams and modern peat formations.

The important memoir on coal by Grand' Eury[1] contains

[1] Grand' Eury (1).

a considerable amount of information as regards the microscopic structure; he discusses the source and manner of formation of the mineral charcoal (Fusain or Faserkohle), and considers that it has been formed by the breaking up of the woody interiors of many of the flattened hollow stems which are so common in coal and the associated shales. The separation of bark and wood, and the gradual disintegration of the latter, are phenomena which may be observed in present-day forest trees.[1] In amorphous coal, Grand' Eury considers we have the result of a precipitation of ulmic substances, with spores and other parts of plants on the floor of a lake or sea. In their recent monograph on the fossil plants of the Commentry coal field, Renault and Zeiller devote some pages to the microscopical examination of coal. The cannel coal of Commentry is compared with that from Lancashire; both consist largely of an amorphous substance, with occasional spores and tissue fragments.

Anthracite is more difficult to examine, and shows less organic structure. The boghead is rich in inorganic matter, but also contains numerous small lenticular bodies exhibiting fine radiating lines, extending from the centre to the periphery, where they lose themselves in a mass of fine granulations.[2] In ordinary coal portions of carbonised stems of *Calamites*, *Psaronius* and other plants are occasionally met with.

Allusion has already been made to the Scotch torbanite or boghead; this, with similar carbonaceous beds from Autun and Australia, has recently been the subject of an article by Bertrand, who has previously published some researches on the same subject in collaboration with Renault. The bogheads are described as coals which yield, on distillation, a large quantity of very bright gas. Examined microscopically they reveal the existence of numerous golden yellow balls possessing a radiate structure; occasionally there may be as many as two hundred and fifty thousand to one million of these minute spherules in one cubic centimetre. A close examination of these structures leads Bertrand and Renault to regard them as the thalloid bodies

[1] Solms-Laubach (1), p. 24. [2] Renault and Zeiller, Pl. lxxiv., fig. 9.

of a gelatinous alga, to which the name *Pila bibractensis* has been assigned. The thallus is divided up into a number of small thick-walled cells, and in some of the cell cavities—thanks to their wonderful preservation in silica—protoplasm and nuclei have been recognised. We may be permitted to express grave doubts as to the possibility of such well-nigh incredible statements. In addition to the numberless examples of *Pila bibractensis*, pollen grains are not uncommon. The algal thallus and pollen grains are embedded in a brown ground mass which constitutes a kind of amorphous precipitate charged with vegetable fragments. The conclusion arrived at is that the Autun boghead is the product of an immense accumulation of a single species of gelatinous alga, with grains of pollen and other plant structures, in a matrix of ulmic substances. The brown colour suggests the coffee-coloured waters of some tropical rivers, and the algæ may be regarded as analogous to the *fleurs d'eau* of fresh-water lakes. In the calm, brown waters of a Permo-Carboniferous lake, ulmic materials were precipitated by the action of carbonated waters; and at certain seasons of the year the surface of the water was covered with a mass of microscopic algæ, and these, with showers of pollen from neighbouring forests, accumulated as a pulpy mass of ulmic products on the flora of the lake, and so gave rise to a deposit of boghead. A similar structure is recognised by these authors in an Australian boghead, and in the torbanite of Scotland. In the Australian boghead *Reinschia australis*, another gelatinous alga, has played the most important *rôle* in building up the carbonaceous material; and in the Scotch rock another species of *Pila* is the characteristic constituent. In addition to the algal species, an aquatic Myxomycete is recorded from the Autun boghead, described under the name of *Bretonia Haidingeri;* the same genus has also been found in the Scotch beds.

It should be noted that the Autun bogheads may occur mixed with ordinary coal, and that coal is sometimes found in the form of lenticular patches in a bed of boghead. The results arrived at by Bertrand and Renault compel us to adopt a somewhat sceptical attitude in attempting to form an

opinion as to the facts they record. We are accustomed to find in the petrified remains of Permo-Carboniferous plants every detail of wall sculpture and cell outline faithfully preserved, but to have millions of examples of a gelatinous alga with cells, and even cell contents, clearly defined, is a revelation which borders on the miraculous.

Have we sufficiently good evidence before us that the boghead structures are really gelatinous algæ? Must we look upon the bright, yellow bodies in these rocks as the result of the phenomenon which we are familiar with on a smaller scale in the breaking of the meres? In the first place it may fairly be asked: Do the descriptions and figures of *Pila bibractensis* show a marked resemblance to any known form of alga? Bertrand gives numerous drawings of this species, showing what he believes to be different stages in the development of the thallus. The general appearance of the structure does not suggest any distinct resemblance to any type of recent plant, and it is difficult to understand with what form of alga these Palæozoic specimens may be best compared. The existence of protoplasmic and nuclear substances in a fossilised condition, as described by Bertrand and Renault in the case of the boghead structures, and by Lignier[1] in Bennettites, can hardly be credited; it is true we frequently find a black substance in the secretory canals of fossil plants, but the preservation of carbonised resins or gums is much more intelligible than the mineralised remains of protoplasmic material. It is hoped than an examination of boghead sections may lead to a more definite expression of opinion on the nature of these cellular structures, but at present it is very difficult to accept the published results as to these minute yellow-coloured bodies. Without venturing to speak at all dogmatically, the more probable conclusion seems to be that we have here to deal with curious inorganic structures which closely simulate the cellular structure of plants.

A. C. SEWARD.

(*To be continued.*)

[1] For notice of Lignier's excellent paper see *Nature*, p. 594, Oct., 1894.

THE COAGULATION OF THE BLOOD.

THE causes that lead to the clotting of blood form a subject which seems to possess a peculiar attractiveness to the investigator of physiological problems. Like the subject of muscular contraction it produces every few years a fresh crop of theories seeking to explain it. But in both cases, the new facts that are discovered often throw fresh difficulties in the way of, instead of shedding new light upon, the vexed question at issue.

It is, however, useful to pause every now and again, and take stock of the scientific position in matters of this kind. We must start by frankly acknowledging that a final and conclusive theory has yet to be discovered, but a historical retrospect is by no means uninteresting and is often useful. I therefore propose in the following article to sketch briefly the story of blood coagulation in the hands of scientific investigators, and to compare our present knowledge with that of our predecessors. I think it will be acknowledged that we have made some progress of late years, but the progress has consisted rather in discovering our ignorance than in removing it.

When the microscope first came into use, it was recognised that the blood is not a homogeneous red fluid, but consists of a nearly colourless fluid, the plasma, in which are suspended a number of particles which are called the blood corpuscles. Of the two main classes of corpuscles, the red ones are by far the more numerous, and give the red tint to the blood as a whole. The white corpuscles are small and typical animal cells, masses of living material (protoplasm), containing a nucleus.

In those early days, the clot which occurs in blood after it is shed was supposed to consist merely of a mass of adherent corpuscles. Some held that they stuck together because the blood was no longer in active movement; others again thought the cooling of the blood after its removal from the body caused the corpuscles to form a coherent jelly, much in the same way that soup sets when

it is cold. We now know that agitation hastens and does not hinder coagulation, and that cooling hinders and does not hasten the process. We have, moreover, learnt that the clot contains something else in addition to the corpuscles; this something does not exist as such in the living blood, and it is of the nature of an insoluble proteid or albuminous matter. It is called fibrin, and, as its name implies, it consists of fibres or strings which bind the corpuscles together. The essential fact in coagulation is the formation of fibrin, and the causes of coagulation narrow themselves down to the causes of fibrin-formation or precipitation.

It was not until the close of the eighteenth century that an idea of a coagulable substance in addition to the corpuscles was mooted; the existence of fibrin was fully recognised by Hewson (1772), and taught by Fordyce and the Hunters.

Hewson obtained specimens of blood which coagulated with great slowness; when these were allowed to stand, the corpuscles settled towards the bottom of the containing vessel, leaving a clear layer of plasma at the top. This he skimmed from the surface, and found that after waiting a short time the strands of fibrin were slowly deposited till the whole fluid had set into what looked like a jelly, so close were the meshes of the network. Hewson further discovered the fact that cold, contact with living blood-vessels, and admixture with certain neutral salts are agencies which hinder or prevent coagulation by delaying or preventing the deposition of fibrin filaments. In connection with the influence of the living vessels on the process, the subject was at a later stage taken up by Lister, Fredericq and Brücke, who worked out many of the details in connection with this inhibitory influence.

Andrew Buchanan of Glasgow appears to be the next who made noteworthy investigations on this subject. He experimented with fluid obtained from the pericardial sac and from the tunica vaginalis in the dropsical condition of that serous membrane called hydrocele. These liquids resemble blood plasma very closely, but they do not coagulate spontaneously; Buchanan found that the addition

of small shreds of "washed blood-clot" caused the formation of fibrin in them. This power was exhibited to a still greater extent by the "buffy coat" of a clot; he therefore concluded that the power resided in the white blood corpuscles which are so abundant in the buffy coat, and with almost prophetic instinct, though he did not employ the term ferment, he compared this action to the action of rennet in curdling milk.

British investigators having cleared the way, we find the next important names among the workers on the Continent. First came Denis, who saturated blood plasma with sodium chloride, and thus obtained a proteid precipitate. This precipitate was collected, re-dissolved in a little water, and allowed to stand. The aqueous solution remained liquid for a short time, but soon a clot of fibrin made its appearance. Denis had thus obtained from the plasma the soluble precursor of the insoluble fibrin, and he named it plasmine. Denis' plasmine was soon shown to be a mixture of at least three substances, and to them the term fibrin-factors was, until quite recently, universally applied.

Alexander Schmidt, Professor at Dorpat, recognised these three materials and named them fibrinogen, fibrinoplastin, and fibrin-ferment. He considered that the two first-named substances, which belong to the globulin class of proteids, united together and formed fibrin, and the agency which caused their union was the fibrin-ferment.

Olaf Hammarsten, of Upsala, ascertained the characters of these substances with greater exactness, but his most important contribution to science was the discovery that the fibrinoplastin (or, as it is now called, paraglobulin or serum-globulin) was not essential. A solution of fibrinogen *plus* ferment will cause the formation of fibrin: the paraglobulin always remains, if present, in solution, though its presence hastens coagulation; this faculty it was found to share in common with casein from milk, and even the inorganic salt calcium chloride.

The question of the causation of blood clotting had now narrowed itself still more. It was fibrin-ferment which caused fibrin formation; the cause of the formation of

fibrin-ferment in shed, that is, in dead or dying, blood had next to be discovered. Here Schmidt and Hammarsten were agreed, and their lead has been followed by the greater number of subsequent investigators, that it was the disintegration of the colourless corpuscles which led to the shedding out of this new material.

The whole theory so propounded may be put briefly as follows: When the blood is within the blood-vessels, one of the constituents of the plasma, a proteid of the globulin class called fibrinogen, exists in a soluble form. When the blood is shed, the fibrinogen molecule is split up, the comparatively insoluble substance, fibrin, being the principal product of its disintegration. This change is brought about by a special unorganised ferment called the fibrin-ferment, which does not exist in healthy blood contained in healthy blood-vessels, but is one of the products of the disintegration of the white corpuscles that occurs when the blood leaves the blood-vessels or comes into contact with foreign matter.

Now this was a very good working theory; it possesses the merit of comparative simplicity, and is in accordance with the experimental evidence which was at the disposal of Schmidt and of Hammarsten. It is the theory which is given as gospel in most of the leading text-books on Physiology. But workers all round are beginning to doubt if it is true, or, at least, if it is the whole truth. There can be no doubt that fibrin-ferment prepared by Schmidt's method does cause coagulation in certain forms of plasma, obtained from the blood by preventing it from coagulating by such means as admixture with neutral salt. The weak point in the theory has always been recognised to be the fact that injection of fibrin-ferment into the circulation of a living animal does not cause intravascular clotting. Hence it was necessary to tack on to the theory the postscript that the living vessels possess in some way a power either to counteract the action of fibrin-ferment, or to destroy it.

Looked at from another point of view, too, the theory, after all, only shifts the matter a little farther back, for we have now to ask the cause of the disintegration of the

white corpuscles. This was explained by saying that the white corpuscles are exceedingly sensitive to change; while they are swimming happily round their circle from arteries to veins and from veins back to arteries again they are in their element, and so they do not break up, or only so slowly that the vascular wall is able to cope with the products of their death; but when they get out of their normal habitat, they break down, not in ones or twos, but in battalions, and the innocent fibrinogen falls a victim, and starts a new phase of existence as fibrin. This explanation is, of course, no explanation; it is hardly an apology for one; if we were able to say why the white corpuscles die we should be very near to explaining the mystery of the difference between life and death.

About the time, now nearly twenty years ago, when scepticism of this sort was simmering, some folks began to inquire whether it really is the fact that the white corpuscles break up when the blood dies; and to the late Dr. Wooldridge belongs the credit of pointing out that this fundamental point in the ferment theory rests on very flimsy evidence. Soon one of Schmidt's pupils, Rauschenbach by name, had to admit that there were two classes of white corpuscles, one class consisting of those which did, the other of those that did not disintegrate. Prof. Haycraft, too, was never able to see actual disintegration, though he described certain differences of appearance between the living and the dead leucocyte (white corpuscle). Observations made much more recently in my own laboratory have led me to follow in the wake of these observers, and I feel that the word disintegration must not be taken in the too literal sense in which it was used by Schmidt; it may be that the dying corpuscle sheds out material, though it does not break up into fragments.

Wooldridge thus started a new era in the history of the subject; he, perhaps somewhat hastily, propounded a new theory, part of which will perhaps stand. But whether it does or not matters but little, for, by breaking new ground, and inspiring healthy scepticism, he has succeeded by his own work, and by that of his followers, in introducing new

ideas, new methods of investigation, and new knowledge in consequence.

His work fell into three chief lines :—

1. He attributed to the corpuscular elements of the blood a secondary *rôle* in the causation of clotting. He considered that the *débris* of corpuscles described by Schmidt is analogous to, if not identical with, a precipitate he obtained by cooling peptone plasma.[1]

2. He attributed to a compound rich in phosphorus a very important part in producing fibrin formation ; he thought this compound was lecithin ; but all recent work tends to show that it is nuclein rather than lecithin, that is the phosphorised compound he was dealing with.

3. He discovered a material which, when injected into the circulation of a living animal, does produce intravascular coagulation ; and this material is not fibrin-ferment. This third branch of Wooldridge's work is the one which has borne most fruit.

When these views were first promulgated they met with a good deal of incredulity, and it appeared absolutely impossible to reconcile them with those of the older school of Schmidt and Hammarsten. Pekelharing was the first to attempt to reconcile the conflicting theories ; and he fancied he had discovered the connecting link in the relationship of calcium salts to the coagulation process.

Many years ago, Brücke demonstrated that the ash of fibrin always contains calcium. Later (1875), Hammarsten, as we have already noted, pointed out that calcium chloride can take the place of serum-globulin in aiding the action of fibrin-ferment. In 1887, Green found that the coagulation of various forms of plasma is much accelerated by small quantities of calcium sulphate ; and then Drs. Ringer and Sainsbury showed that the same property is possessed by other calcium salts, and to a less extent by the salts of strontium and barium. Freund, who had made somewhat

[1] Peptone plasma is plasma obtained from blood by injecting into the circulation of the animal before death a solution of commercial peptone. The active ingredients in the so-called peptone appear, however, to be albumoses.

similar observations, formulated a new and eminently simple theory of blood coagulation, which was almost immediately annihilated.

The next to enter the field were Arthus and Pagès, who made the brilliant discovery that the blood may be kept liquid by decalcification. This may be readily brought about by mixing the blood immediately it is shed with a 0·2 per cent. solution of potassium oxalate. They consider that fibrin is a calcium compound of fibrinogen, and their experiments further led them to believe that the ferment as well as the calcium salt is necessary for the transformation of fibrinogen into fibrin.

Green, in the work already alluded to, took up the question whether the fibrin-ferment exists in the blood as a zymogen (mother of ferment) which is changed into the ferment by the calcium salt. His conclusions, however, were negative. This point was taken up by Pekelharing, who prepared from various forms of extravascular plasma a substance with the solubilities of a globulin which possesses no fibrinoplastic properties, but which, by treatment with a calcium salt, assumes the fibrinoplastic activities of fibrin-ferment.

He therefore regards it as the mother substance of the ferment, and as identical with a substance I myself had previously described as cell-globulin. Pekelharing considers with Arthus and Pagès that fibrin is a calcium compound of fibrinogen, and that the main action of the ferment is the handing over of the calcium to the fibrinogen. Oxalates hinder coagulation because they precipitate the necessary calcium salts ; and there is very good reason for believing that peptone acts in a similar way for a similar reason, namely, its affinity for calcium salts.

He then proceeded to examine the "tissue fibrinogens," the substances which Wooldridge discovered to be capable of producing intravascular coagulation. Like other observers he found their chief constituent to be nucleo-albumin ; and in a later research he discovered that the globulin just alluded to, my cell-globulin, was also a substance of the same nature, yielding an insoluble residue of

nuclein on gastric digestion. In other words, the substances which have at various times received the names of fibrino-plastic substance, fibrin-ferment, cell-globulin, fibrinogen A., tissue fibrinogen, etc., etc., are all varieties of one substance which is nucleo-albumin, and further, that it is in all cases a nucleo-albumin which, in co-operation with calcium compounds, brings about coagulation in the blood.

In order to make this review complete, it is now necessary to allude to the recently-published researches of three more observers or sets of observers.

Wright has devoted himself to what may be an important side issue, namely, the influence of the amount or the tension of carbonic anhydride in the coagulating blood. It is certainly a fact that intravascular coagulation is generally most intense in the veins, where the pressure of this gas is greater than in the arteries.

Lilienfeld and Kossel, in Berlin, have also turned their attention to nucleo-albumins, and have dubbed them nucleo-histons, on account of a supposed resemblance between histon, the proteid moiety of the material, and peptone.

And lastly, in conjunction with Dr. Brodie, I have recently published in the *Journal of Physiology*[1] an account of some experiments in the same direction, which we have been carrying on at King's College for the last three years; and I propose to conclude this paper by giving a brief summary of our results.

1. Nucleo-albumins may be prepared from most of the cellular organs of the body (muscle is an important exception) either by Wooldridge's acetic acid method, or by a new method in which alternate treatment of the tissue with sodium chloride and water constitutes the main feature, but for full details of which the original paper must be consulted.

2. The material obtained by both methods from the same organ is the same in (1) general reactions which closely resemble those of a globulin, (2) percentage of phosphorus, and (3) physiological action, *i.e.*, the production of intravascular coagulation; death is due to cessation of

[1] Vol. xvii., p. 135. References to other authors will be found in this paper.

respiration primarily caused probably in the respiratory centre. The nucleo-albumins, obtainable from various organs, differ in some minor points.

3. Protagon, the most abundant impurity in these preparations, and the closely-related substance lecithin, are not responsible for the clotting; neither is the dilute sodium carbonate which was used as a solvent.

4. The nucleo-albumins do not accelerate the coagulation of extravascular (dilute salted) plasma, and so contrast very forcibly with fibrin-ferment.

5. A few experiments with "peptone" and "leech extract" confirm the hypothesis of Pekelharing, that these substances hinder coagulation on account of their affinity for calcium.

6. Our experiments lend no support to the theory of Wright and Lilienfeld, that the negative phase of coagulation (*i.e.*, the production of non-coagulable instead of coagulated blood) sometimes observed is produced by the splitting off of a peptone-like substance from the nucleo-albumin. There is no evidence of peptone or albumose in the blood or urine of the animal under experiment, and the properties of the albuminous moiety of nucleo-albumin are entirely unlike those of peptone.

7. Failure to produce intravascular clotting is partly explained by idiosyncrasies of the animals used, and partly by certain factors in the preparation of the nucleo-albumin; these are discussed with full details and tables of analyses.

8. There appears to be some evidence that the nucleo-albumins active in producing clotting are preceded in the cells themselves by similar substances which differ from them in not possessing this remarkable physiological activity.

9. Solutions of nucleo-albumins destroy the blood corpuscles. This, however, is chiefly due to their alkalinity, and will not explain the thrombosis (intravascular clotting) produced. Distilled water, for instance, is a powerful solvent of blood corpuscles, but never produces intravascular coagulation. Löwit has recently given a list of agents that produce leucolysis (destruction of leucocytes) and states that *plus* calcium chloride they always cause thrombosis. We have repeated these experiments carefully, but were en-

tirely unable to corroborate Löwit's statement. Schmidt's view of the preponderating influence of leucolysis on blood-clotting is therefore not borne out.

I should like to add in conclusion that, although I regard Pekelharing's attempt to harmonise rival theories as extremely ingenious, it appears to me to break down in two points. The first of these is his idea that nucleo-albumin is the mother substance of fibrin-ferment, and convertible into fibrin-ferment by the action of a calcium salt. His experiments do not bear this out thoroughly, for the amount of calcium salt is presumably the same in extravascular as in intravascular blood. Yet nucleo-albumin causes coagulation in one, but not in the other.

The second point is his inclusion of fibrin-ferment among the nucleo-albumins; there is no analytical evidence of this.

It is quite possible that nucleo-albumin and fibrin-ferment are related substances, perhaps related very closely. It is also quite possible that they are absolutely different substances. There certainly are differences: let me enumerate some.

a. Fibrin-ferment is not readily coagulated by alcohol; nucleo-albumin is.

b. Fibrin-ferment cannot be obtained by Schmidt's method from nucleo-albumin.

c. Fibrin-ferment causes coagulation in extravascular (salted) plasma; nucleo-albumin does not.

d. Nucleo-albumin causes coagulation in intravascular blood; fibrin-ferment does not.

These last two differences form one of the greatest difficulties in properly understanding the method of fibrin-formation, or at least of reducing it to one common law.

Why, after all, is there a need for a common law? May not the two substances in question be quite distinct from each other, each being capable of producing fibrin under suitable conditions?

It is by no means inappropriate to close a paper on such a subject as blood coagulation with a number of unanswered questions.

W. D. HALLIBURTON.

INSULAR FLORAS.

PART III.

IN this part I propose passing in rapid review the most important botanical literature of the last decade relating to the arctic islands, more particularly those of the eastern hemisphere, and the islands of the Atlantic Ocean. In succeeding articles I intend taking the West Indies and the African islands of the Indian Ocean, followed by a brief examination and discussion of the facts and theories bearing on insular floras generally, especially in relation to the new lights of recent discovery and investigation. For this purpose I am desirous of adding to the bibliography any publications of moment that I may have omitted, and I should be thankful to my readers for any information they can give me on this point. Botanical literature has increased so rapidly during the last ten years, and is so widely scattered, that important contributions may easily be overlooked, even when one has the advantage of the most extensive libraries. Therefore my request.

The bibliography alone of the botany of the islands of the arctic and sub-arctic seas would fill several pages, even if we only go back as far as the last British Polar Expedition. I will therefore confine myself to a few of the principal publications in which students will find references to the less important ones. We are chiefly indebted to Scandinavian botanists for the more complete investigation of the arctic islands of the eastern hemisphere, and for some exceedingly interesting discussions on the history of the arctic flora generally. It is true that much of this literature deals with the origin of the flora of Greenland, which hardly comes within the scope of the present paper. The "Vega" and "Dymphna" Expeditions were especially fruitful. Among the islands to the extreme east of Asia recently visited are several of the Aleutian chain, on the Asiatic side of the strait, including Behring Island and two or three others, known collectively as the Komandarski or Com-

mander group (1); the name being very variously spelt. This is little more than a list of 132 species of vascular plants collected in Behring and Copper Islands, but it is supplemented (2) by a brief account of the plants collected in Behring Island by the "Vega" Expedition. Although in no higher latitude than 55°, or the same parallel as Newcastle, there is no arboreous vegetation, and the shrubs are few and small or stunted, being overtopped by tall herbaceous plants, characteristic of the grassy plains (3) of Kamtschatka. There is, perhaps, no cold country in the world where herbaceous plants attain such an astonishing size as in Kamtschatka, and although the insular plants do not attain equally large dimensions, Kjellman states that the shrubby vegetation is concealed by herbs commonly as tall as a man. Conspicuous among these are: *Cacalia auriculata*, *Senecio palmatus*, *Cirsiumkamtschaticum* (*Compositæ*), *Conioselinum kamtschaticum*, and *Heracleum lanatum* (Umbelliferæ). Species of many other genera grow to an unusual size, for example: *Pedicularis*, *Polemonium*, *Sieversia*, and *Aconitum*. Woody plants are represented by *Pyrus sambucifolia*, *Rhododendron chrysanthum* and *Salix arctica*. The plants named are characteristic of the slopes towards the sea; but the interior plateau has a heath-like vegetation consisting of *Bryanthus Gmelini*, *Cassiope lycopodioides*, *Arctostaphylos alpina*, and similar plants. Characteristic arctic plants are wanting, and Kjellman designates the majority of the species as arctic-tertiary, which probably formerly had a much wider distribution.

St. Lawrence, in the mouth of the straits, is another of the islands botanised (4) by the "Vega" Expedition; and the flora of the Western Esquimaux-land (5) is interesting for comparisons. A fragment of the tertiary flora of the islands of New Siberia (6) leads the author to the conclusion that they have been separated from the continent in comparatively recent times. The coniferous element predominates, and associated with it is a brittle kind of amber, such as occurs on the mainland. *Sequoia*, *Dammara*, *Taxodium*, *Pinus*, and *Cupressinoxylon* are the genera recognised.

Novaia Zemlia has been by this time so exhaustively explored that little can remain undiscovered ; and the same may be said of Spitzbergen. Th. Holm has brought together and discussed in some detail (7) the composition and physiological characters of the vegetation of the former country. He also gives a tabular view of the distribution of the 193 phanerogams and four vascular cryptogams hitherto collected, and indicates their possible migrations. His table exhibits the following extensions : Arctic America, Greenland, Iceland, Jan Mayen, Spitzbergen, Bear Island, Scandinavia, Arctic Russia, North Siberia, and the Asiatic coast of Behring's Sound. Out of 193 species, 133 are common to Greenland and 113 to Behring's Strait. The plants new to science are : *Colpodium humile*, *Calamagrostis Holmii*, *Glyceria tenella* var. *pumila* and a hybrid willow—*Salix arctica* × *polaris ;* and those new to Novaia Zemlia : *Cineraria frigida*, *Potentilla emarginata*, *Epilobium alpinum*, *Draba repens*, *Ranunculus affinis*, *Alsine biflora*, *Carex incurva*, *C. lagopina*, and *C. hyperborea*. The predominating natural orders are : Gramineæ, 31 species ; Cruciferæ, 21 ; Cyperaceæ, 20 ; Compositæ and Caryophyllaceæ, 14 ; Salicineæ, 13 ; and Saxifragaceæ, 10. Petaloid monocots are limited to *Allium sibiricum* and *Lloydia serotina ;* no orchid having been found.

As already indicated, the literature relating to the flora of Spitzbergen and Greenland is voluminous ; and although some of it is rather earlier in date than I proposed to attempt to deal with, it seems desirable to make some reference to it, because there is an intimate connection between that and the later contributions ; Nathorst and Warming having entered into a critical dispute on the age and origin of the flora. Nathorst's original work in Swedish (8), of which there is a German abstract (9), is a very elaborate performance ; the local and general distribution of the plants being most fully tabulated, in order to prove the direction of migration. The author's principal conclusions may be given in all brevity, and without comment. First he points out that the [vascular] flora of Spitzbergen is richer than that of any other country in the same latitude ; and many of the

species are so rare and local that he predicts the discovery of several more species. It should be remembered that the eightieth parallel traverses the north-east island. He was of opinion, too, that most, and probably all of the Spitzbergen plants, have migrated thither during the post-glacial period. About seventy-five per cent. of the vascular plants are described as flourishing perfectly and producing seeds, and these, it is assumed, reached the islands before the remaining twenty-five per cent., which consist mainly of bog and sea-shore plants. The migration of these plants into Spitzbergen was, probably with few exceptions, overland, as Spitzbergen and Novaia Zemlia were connected with Russia and Scandinavia, the connection having since subsided. Nathorst further argues that there have been none but accidental exchanges between the floras of Greenland and Spitzbergen in post-tertiary times. This argument is further developed in several articles (10) in which the author analyses the flora of Greenland in detail; and Warming (11) contests in detail the correctness of Nathorst's theories. Schenk (12) gives a short account of some fossil woods from Green Harbour, figured by Heer (13), which Schenk regards as jurassic rather than tertiary. The interesting fact connected with this is the former existence of arboreous vegetation in so high a latitude as 78°. I can only refer to the reports on the botanical results of the last British Polar Expedition (14), and Lieutenant Greely's Expedition (15); and I merely introduce them in order to render the bibliography more nearly complete for purposes of comparison. Mr. Hart's work is a most valuable synopsis of facts.

Proceeding southward into the more open ocean, we come to the island of Jan Mayen in latitude 71°, and about 150 miles from the coast of Greenland. It is some thirty nautical miles in length, and rises in the north-east to a height of between 6000 and 7000 feet. The central part is a narrow neck of land connecting the northern and southern expansions. Drift-wood Bay, on the eastern side of this neck, received its name and is remarkable for the quantity of drift-wood found there. Until the Austrian expedition

visited Jan Mayen, next to nothing was known of its botany. A Norwegian expedition visited the island in 1877 and collected the following plants : *Ranunculus glacialis*, *Cerastium arcticum*, *Draba corymbosa*, *Cochlearia grœnlandica*, *Halianthus peploides*, *Saxifraga cæspitosa*, *S. nivalis*, *S. oppositifolia*, *S. rivularis*, *Oxyria digyna* and *Catabrosa algida*. Dr. Fischer, of the Austrian expedition (16), collected a considerable number of cellular cryptogams, and added the following vascular plants to those enumerated above : *Ranunculus pygmæus*, *Cardamine bellidifolia*, *Draba alpina*, *Silene acaulis*, *Saxifraga cernua*, *Taraxacum officinale*, *Mertensia maritima*, *Salix herbacea*, *Koenigia islandica*, *Polygonum viviparum*, *Luzula arcuata*, *Poa alpina*, *P. flexuosa*, *Festuca ovina*, *F. rubra*, *Cystopteris fragilis* and *Equisetum arvense* ; making a total of twenty-eight species. As only certain localities were botanised, it may be that a few more species yet remain undiscovered ; but there is no gainsaying the extreme poverty of the flora, as compared to that of Spitzbergen ; and all the species are of wide distribution. Some of them are very rare in the island, or only represented by scattered individuals. The five species of *Saxifraga* are among the most generally dispersed plants, and *Ranunculus glacialis* is the showiest of all. The last bedecks the Alps of Europe as well as the arctic regions of the Old World and America. Dr. Fischer made a collection of drift-woods, which have been worked out (17) ; but I must be content with giving the reference.

Since the appearance of Grœnlund's "Flora of Iceland" in 1874 (18), in which he gave a sketch of the history of botanical discovery in the island, there have been several important contributions, notably one by himself (19) dealing with the composition and general geographical distribution of the elements of the flora. A critical examination of the work of his predecessors led to a considerable reduction in the total number of species believed to inhabit the islands ; the result being 340 species of phanerogams, and 26 species of vascular cryptogams. Not one species is peculiar, and out of a total of 366 species, 360 are common to Scandinavia proper. Fifty-nine natural orders are represented ;

twenty-one of them by only one species each. Brightly-coloured flowers are not wanting, though in number of species those having inconspicuous flowers largely predominate. Thus, of the Cyperaceæ there are 41 species; of the Gramineæ, 36; and of the Juncaceæ, 18. With the exception of the Cruciferæ (21) species, all the others are below 18. Various other botanists have subsequently taken up the subject, and Grœnlund's work has been traversed, criticised, and amended in details. An English summary of this exists (20). There is also a further contribution (21) by a native of Iceland on some new or rare vascular plants from the island. The latest English summary (22) of the flora is an interesting contribution to this literature, though I believe it is little known. The author gives the results of his own collections and impressions, both botanical and entomological, and many are of far more value than one ordinarily meets with, especially those relating to the predominance of certain plants in certain districts; the colour in the landscape; the characteristic alpine, moor, and marsh plants; the plants—*Thymus Serpyllum* and *Parnassia palustris*, that flourish in closest proximity to the hot springs and steam of the geyser; and the plants that are generally distributed in the island, illustrated by comparisons with the conditions and phenomena in other countries. He gives, on the authority of Mr. Arthur Bennet, the total number of species of vascular plants at about 428; but the estimate depends largely upon the view taken of specific limits. Mr. Walker himself collected only eighty-two species; but he separately reproduces the list of 477 species enumerated in Baring-Gould's book on Iceland—a list evidently requiring some revision. Mr. Walker's observations on the entomology of Iceland are equally interesting, especially in relation to the fertilisation of flowers. He states that the leading characteristics are: Total absence of butterflies and *Orthoptera*; *Neuroptera* only represented by *Phryganidæ*; and most of the moths are of a dusky colour, in harmony with the lichens and rocks. Moths and Diptera, he adds, appear to take the place on flowers that butterflies and Hymenoptera do in Britain.

For the latest account of the vegetation of the Færoes, we are indebted to two English ladies (23). Only three of the islands were explored, namely: Stromoe, the largest, which is twenty-seven miles in length and seven miles in breadth; Naalsoe and Osteroe. Rostrup (24) records 307 flowering plants from the islands, only five of which do not, on his authority, occur in the Scandinavian peninsula, namely: *Alchemilla fissa*, *Anagallis tenella*, *Myosotis repens*, *Scilla verna* and *Carex Lyngbyei*. Further, only ten of them are not found in Britain, and these are almost all strictly alpine in character. As in St. Kilda, trees are entirely absent, and the shrubs of the dwarfest habit. Miss Copland and her companion collected only about a third of the plants, but their sketch of the general character and aspects of the vegetation is most interesting.

The flora of St. Kilda, the most westerly specks of land in Great Britain, is still imperfectly known, though there is an interesting recent contribution to the subject (25). St. Kilda is not so familiar that I need apologise for giving a few facts concerning the group; for there are several islands. They lie between fifty and sixty miles west of the Outer Hebrides. The largest island, bearing the name given to them collectively, is about three miles long by two broad, and the highest point some 1220 feet above sea-level. So far as the botany is concerned, no species has been found on any of the smaller islands that does not occur on the main island. There is not a native tree, and the shrubby vegetation is limited to such plants as *Vaccinium Myrtillus*, *Erica cinerea*, *Calluna vulgaris*, *Empetrum nigrum*, and *Salix repens*; consequently, the number of species comprising the flora is very small. Yet there is pasturage sufficient to support considerable flocks of sheep. It includes in its composition over a dozen kinds of grasses, common thyme in abundance, and white clover. The total number of species of vascular plants recorded is 120, several of which, however, are exceedingly rare, or only grow in the barley and oat crops, thus bringing down the probably indigenous species to about 100; a number that is equalled on an acre of ground in the South of England. Genuine alpine plants

are almost wholly wanting: *Silene acaulis*, *Saxifraga oppositifolia* and *Oxyria digyna* are the nearest approach to this class of plants.

A catalogue of the phanerogamic plants of Madeira and Porto Santo (26), not included in the unfortunate Lowe's unfinished flora of Madeira and the neighbouring islands, will be found useful, though it only causes us to lament the more the untimely end of that author. This catalogue contains naturalised plants and a few notable cultivated ones, with remarks on the local distribution of the indigenous species, many of which are exceedingly rare. The following are described as new:—

Koniga arenaria, *Scrophularia Moniziana*, *S. Johnsoniana*, *S. maderensis*, *S. oblonga* (name only), *Romulea juncea*, *Tinantia fallax*, *Potamogeton cuprifolius*, *P. Machicanus*, *Phalaris altissima*, and *Sesleria elegans*.

The Canary Islands have furnished facts and figures for the phytogeographer from almost the earliest investigations of the distribution and migration of plants. Humboldt and other fathers of the science drew largely at this fountain; and their labours together with Webb and Berthelot's great descriptive work might have been considered exhaustive, yet Dr. Christ (27) has collected data for one of the most interesting of recent essays on geographical botany. Dr. Christ's work is the result of personal observations, and it is very much more than a statistical exposition of the components of the flora. Biological phenomena receive due attention, as is exemplified by the discriminating manner in which he describes the vegetation as distinguished from the flora. This work may be profitably studied in connection with Dr. Balfour's flora of Socotra (28), in which the author draws some striking comparisons of the relationships between the floras of these two distant insular regions. Primarily, Dr. Christ insists upon a close relationship between the floras of the Azores, Madeira, the Canaries, and the Cape Verd Islands, and treats them as parts of an intimately connected whole. Apart from the last, there is no doubt that the predominating elements are much the same in these four groups of islands, which are scattered through twenty-five degrees of latitude from 15° to 40°. But although there is

a large temperate element in the Cape Verd Islands, considering their tropical situation, and a subtropical element in the Canaries, there is hardly that homogeneity throughout to warrant the conclusion that they are actually separated fragments of one and the same flora, as distinguished from the flora of the nearest continental regions. It is true that, taking the Canaries as the centre, and the north and south groups as outliers, there are very evident connections, which Dr. Christ finds even "really surprising" in the case of the Cape Verd and Canaries; but before entering into details of this part of the question it may be well to give some of Dr. Christ's facts and conclusions. It should be remembered that the Canaries are the nearest to the continent of the four groups of islands or archipelagos—Azores, Madeira, Canaries, and Cape Verd; and the islands Lanzarote and Fuertaventura are much nearer than the rest of the Canary group, being only about 1° distant. The peculiar element in the flora of the latter being comparatively small, Dr. Christ would prefer designating them continental in contradistinction to the five western essentially oceanic islands. But this is an unnecessary and untenable distinction, for after all none of these islands are oceanic in the sense that St. Helena, Tristan da Cunha, Amsterdam, Rodrigues, the Galapagos, and the Sandwich Islands are. Nor is the endemic element of the same pronounced character as in most of the islands named, being more comparable to the differences existing in some continental areas, in places no more distant from each other than the Canary Islands are from each other and the mainland. Confining ourselves to the African flora, a more strongly marked differentiation will be found to exist in South Africa, within an area extending over four or five degrees of longitude or latitude. Indeed, a great deal too much has been made of the assumed extreme differentiation exhibited by insular floras as opposed to continental floras. Even the distinctive habit of the succulents and the half-shrubby plants that replace the truly herbaceous element in more humid climates, on which considerable stress is laid by Dr. Christ, is not greater than in South Africa.

Returning to the flora of the Canaries, Dr. Christ dis-

tinguishes three zones, or regions, as he denominates them; namely, the coast, cloud, and uppermost regions. The coast region includes the "barrancas" or ravines, and the whole of the cultivated part of the country ranging from the sea-shore up to an elevation of about 3000 feet, and consequently, it is here, too, that the colonised plants are found. Contrary to what has happened in St. Helena and many other islands, introduced plants have made comparatively little way in the Canaries, and, indeed, in the Azores and the Cape Verd Islands—that is to say, beyond the coast region. It is true that the formerly extensively cultivated cochineal cactus (*Opuntia*) has over-run the subtropical part of the country; but what was formerly the chief source of income now remains as a scourge; being one of the few plants capable of competing with the indigenous vegetation. The endemic palm, *Phœnix canariensis*, the aloe, the dragon's blood tree, and the cactus-like shrubby and arboreous euphorbias are conspicuous features in the landscape. *Tamarix canariensis* is the prominent shrub or small tree of the sandy sea-shore; but the endemic *Plocama pendula*, a Rubiacea, with the habit of a casuarina, and various cactus-like species of *Euphorbia*, are the most striking and characteristic of the inhabitants of the succeeding rocky country. The "Cardon," *Euphorbia canariensis* forms dense clumps five or six feet high, consisting of numerous thick, fleshy, angular, prickly stems springing from the same root. But the commonest and tallest of this genus is the "Tabayba," *Euphorbia Regis Jubæ*, occupying the driest situations in the Western Canaries, sometimes growing to a height of twenty-five feet, and forming densely-branched hemispherical masses. Independently of the paragraphs in the essay cited, Dr. Christ has a special article on the Canary species of Euphorbia (29). The famous "Dragon's Blood Tree" (*Dracæna Draco*) is, or rather was, an even more striking feature in the endemic vegetation of the barrancas.

With regard to the great age formerly attributed to this colossal monocotyledonous tree (*Dracæna Draco*), Dr. Christ's measurements confirm the opinion expressed by the writer (30). There is no doubt now that it is of exceedingly rapid growth. A trunk of one growing at Icod los

Vinos was 9·5 metres in circumference at 2·5 metres from the ground in 1857; and this had increased to 11·7 metres in 1884. Shrubby and half-shrubby Compositæ, and numerous species of the boragineous genus *Echium* abound; but the fleshy-leaved Crassulaceæ give character to the vegetation. The latter mostly belong to the type having the leaves arranged in dense, often large, rosettes, from which rise the leafless inflorescences. Nowhere else in the world is there such a concentration of this class of plants; no fewer than fifty-two species being enumerated by Christ, mostly belonging to the genus *Sempervivum*. *S. tabulæforme* forms rosettes as much as fifteen inches across; and, contrary to what is generally supposed, the roots of these succulent plants penetrate the fissures of the rocks to an incredible depth in search of moisture. *Statice* is another prominent genus of perennial duration, being represented by about a score of species. Apart from weeds of cultivation, there is very little truly herbaceous vegetation, and bulbous plants are rare.

The "cloud region" is a zone above cultivation, which is almost constantly enveloped in clouds, engendering a green or leafy vegetation. It is the zone of the laurel forest, consisting of *Persea indica*, *Laurus canariensis*, *Oreodaphne fœtens*, and the much rarer *Phœbe barbusana*. With the exception of the last, which only extends to Madeira, Dr. Christ records these laurels as common to the Canaries, Madeira, and the Azores; but this is probably a slip so far as the *Oreodaphne* is concerned. They are, however, all confined to the Atlantic Islands. *Erica arborea*, *Myrica Faya* and *Pteris aquilina* are the three predominating species in the undergrowth; the two first being both arboreous in the forest itself. Endemic species of *Ilex*, *Myrsine*, *Notelæa*, *Clethra*, *Arbutus*, and *Visnea*, are other noteworthy elements of the vegetation of this zone.

Teneriffe alone has a subalpine region above the forest, with a dry, scorching climate and a thinly-scattered vegetation, consisting largely of *Spartium supranubium*. Herbaceous plants are sparse and of a greyish hue, such as *Viola cheiranthifolia* and *Silene nocteolens*.

Taking Sauer's estimate of 1226 (31), as representing

approximately the number of species of vascular plants growing wild in the Canaries, Dr. Christ would deduct upwards of a third (420) as not being really indigenous; in other words, as not belonging to the original flora. This leaves 806 species, of which 414 are endemic in the Atlantic Islands; and 392 are also continental. About the same percentage obtains in the West Indies; but it is much higher in Mexico, South Africa, and East and West Australia. Still the peculiarity of the Atlantic Islands flora is rather in the habit of the endemic species than in their relative number. By habit I mean vegetative characteristics, which give the endemic plants a vital energy sufficient to enable them to hold their own against all intruders. On this point Dr. Christ has collected a number of highly interesting facts. Grisebach (32) was of opinion that the endemic flora of the Canaries was dying out, and would soon be exterminated by the more vigorous colonists from the continent. Happily, says Christ, this is an error. C. Bolle, the most experienced and exact among Atlantic botanists, pointed out long ago (33) that the native flora would indefinitely survive in spite of cultivation, and the protection afforded by man to introduced plants. The apparently proscribed plants were constantly gathering new strength to recover the lost ground. "In short," Bolle says, "it (the endemic flora) is everlasting, indestructible; and vast tracts of country not forfeited to cultivation are still exclusively left to it." Twenty five years later Dr. Christ found everywhere full confirmation of this statement. In the struggle for existence, he asserts, the local conditions are all in favour of the native plants and against intruders; and so long as the present conditions continue, so long will the present flora flourish. I have already mentioned the great development of the root-system of the succulent plants growing on rocks; and I may add that it is to the great vegetative capacity of the native plants generally that Dr. Christ ascribes their power of resistance to foreign invasion. This applies more especially to the half-woody plants, belonging to genera whose continental species are mostly herbaceous. The huge clumps of rosettes of *Sempervivum*, and the dense hemispherical tufts of *Echium* and *Statice*

are examples of what is meant. Associated with this almost unlimited vegetative power there is a comparatively rare production of flowers, and the flowers are usually small or very small. But the inflorescences, which only succeed each other after long intervals, are commonly very large and very many-flowered ; the outcome of accumulated vital force. Another peculiarity of the Canary flora as an insular flora, and, as Dr. Christ suggests, a proof that it is not the dying-out remnant of a richer flora, is the relatively small number of monotypic genera, and genera poor in species. In this respect, he says, it is quite exceptional ; but the Hawaiian and Galapagos floras also include a number of genera represented by a long series of species. Christ enumerates twenty-six endemic, monotypic genera, or sections of genera, as against twenty-three endemic genera, or sections of genera of more than one species, and fifteen continental genera represented by three or more endemic species.

Pursuing his studies of the Canary flora, Dr. Christ has published (34) some further contributions to the flora, including descriptions of many new forms, mainly from Webb's unpublished manuscript. There is nothing very remarkable among these additions ; but it may be worth noting that Christ describes the plant issued by Webb under the manuscript name, *Todaroa montana*, without any reference to the fact that Bentham and Hooker (35) had discovered that *Todaroa* was a slip of the pen for *Tinguarra*. Appended to the *Spicilegium* is a *Catalogus Plantarum tam Canariis propriarum quam has insulas inhabitantium sed etiam in insulis Azoricis, Maderensibus et Gorgadensibus nec alibi crescentium.* It includes 477 species, with their distribution in the four groups of islands—in fact, the endemic element of the Atlantic islands, as represented in the Canaries.

Since the appearance of Dr. Christ's papers on the flora of the Canaries, Dr. C. Bolle has published a more detailed list of the plants of the islands, Lanzarote and Fuertaventura (36). He fully agrees with Christ in describing the vegetation as very different from that of the other five islands more distant from the continent. Bolle's list includes colonised plants ; but it is a second later paper (37) that more especially claims our attention. This is an historical sketch of

the vegetation of these islands, and an analysis of its composition and relationships. The principal feature in these islands was, and is, the groves of date palms. Bolle regards all the date palms of these islands as the true date of North Africa, *Phœnix dactylifera*, and describes its occurrence in the following words: "Ubiquitaria fere locis idoneis. Sylva miranda, saharienses oases referens, in convalle Rio Palmas. Num *Phœnix canariensis*, Recentiorum quoque in Purpurariis indigena sit, adhuc dubium." On the other hand, Christ writes as though the common wild palm was the *P. canariensis* (*P. Jubæ*), and adds that the continental *P. dactylifera* is extensively cultivated in the islands, where it yields excellent fruit. It is now generally admitted that the indigenous insular form is specifically distinct from the continental one ; yet much uncertainty exists in the various attempts at discriminating the two.

Bolle's analysis of the flora shows an endemic element of thirty-five species ; that is, peculiar to these two islands. Fourteen of these are exceedingly local. He further distinguishes forty-six species as belonging to the characteristic Canary type, and twenty-six to the Sahara type. His catalogue comprises upwards of 400 species, including colonists ; and *Ononis Christii*, *Lotus erythrorhizus*, and *Plantago Aschersonii* are described as new.

Mr. Krause's sketch of the flora of St. Vincent (38) affords material for a comparison of the Cape Verd and Canary floras. The Cape Verd Islands are upwards of 300 miles from the mainland, and in St. Vincent there is a range of hills culminating in the eastern part of the island, in the Green Mountain, at an altitude of about 2500 feet, with cultivation to the summit. There is, in a sense, a wet and a dry season, but prolonged drought is not uncommon. Of arboreous vegetation there is none, and the real shrubby vegetation consists almost entirely of *Tamarix senegalensis* and *Euphorbia Tuckeyana* ; the former, rarely ten feet high, in thickets, on the coast and in the sandy valleys; the latter, sometimes as much as six feet high, common and scattered all over the island, from 200 feet upwards.

Krause's list, compiled from all available sources besides his own collection, comprises only 183 species of vascular

plants; one-third of which he regards as introduced plants. Of the remaining 121 indigenous species many are endemic; but *Tornabenia* (Umbelliferæ) is the only endemic genus; indeed, the only one peculiar to the Archipelago. On the hillsides, from an altitude of about 1300 feet to the summit, the shrubby *Euphorbia Tuckeyana* forms thickets, in some places as much as eight feet high, but usually not more than three. Prominent among the endemic plants associated with the *Euphorbia* are: *Echium stenosiphon*, *Sarcostemma Daltoni*, *Sempervivum Webbii* and *Lavandula rotundifolia*. Altogether, about two-thirds of the species inhabiting the uplands are endemic.

With regard to the affinities and origin of the flora of the Cape Verd Islands, Krause in the main agrees with Christ; but he carefully distinguishes between the two questions. He finds the nearest affinity in the southern half of the Canaries, relying largely on the *Euphorbia* and *Dracæna* elements. *Dracæna Draco* has disappeared from the island of St. Vincent; but is still said to exist here and there in the mountains of St. Nicolao and St. Antonio. But the total absence of the *Genisteæ*, *Laurineæ*, *Chrysanthemum*, *Rhodorhiza*, and *Phœnix*, and the comparative rarity of other characteristic Canary plants, points rather to community of origin than to community of descent. Neither Christ nor Krause will admit of a former land connection of the four groups of Atlantic islands; and the latter regards a former connection of the Cape Verd Islands with the continent as highly improbable. Yet, as before stated, Dr. Christ regards the floras of the islands from the Azores to the Cape Verd as more nearly related to each other than separately to any part of the continental flora. Nevertheless, I agree with H. C. Watson (39) that the flora of the Azores corresponds closely to that of South-western Europe. His list of plants, which occur in the Azores, but not in Europe, and are also common to Madeira or the Canaries, or both, is a very small one, and, as he himself suggests, might be considerably reduced, as it includes a number of plants of wide distribution, among them some that are only colonists in any of the Atlantic islands.

Similarly, I would say that the flora of Madeira, Canaries, and Cape Verd Islands is African, with strong affinities in Eastern North Subtropical Africa.

On the opposite side of the Atlantic to the Cape Verd Islands is the Fernando Noronha group, situated in about 3° 50′ S. lat., and nearly 200 miles distant from Cape San Roque, Brazil, which was discovered by Amerigo Vespucci in 1503. Several naturalists of note have touched there, and collected a few plants, including Darwin in the *Beagle* and Moseley in the *Challenger;* but it was not until 1887 that the islands were thoroughly botanically explored. Aided by a grant from the Royal Society, Mr. H. N. Ridley and Mr. G. A. Ramage spent about six weeks on the islands in that year, and the former published (40) the botanical results of the expedition. These results were somewhat disappointing, because Fernando Noronha had a reputation for insularity which it did not deserve. This was doubtless owing to the fact that both Darwin and Moseley's very small collections of plants contained previously undescribed species ; yet, as it turned out, they had happened to put their hands upon the most striking of the few endemic plants, and Mr. Ridley had few novelties to add. Indeed, there was little to add concerning the general character and origin of the vegetation to that given by the writer (41) in 1884. It is there stated that the flora is quite tropical American, with no greater infusion of peculiar species than would be found in a similar area on the mainland. The new species exhibited no striking characteristics, and it was not probable that further exploration would lead to the discovery of a specially insular endemic element. Nevertheless, Mr. Ridley designates the group as "oceanic," though he had only a few, mostly critical species of well-known genera to add to those previously known. Who first regarded the group as belonging, in relation to the origin of its flora, to the same category as St. Helena, I have not been able to ascertain ; but I find that Dr. Ihering, in an article that is certainly deserving of perusal (42), attributes it to Wallace, and goes so far as to assert that the latter had taken its flora to be primarily of African origin, conveyed by oceanic currents and other means. This seemed to me so

utterly improbable that I wrote to Dr. Wallace on the subject, and he replied to the effect that it must be a mistake on the part of Dr. Ihering. Returning to the composition of the flora, the chief novelties, assuming that they do not exist on the mainland, which is by no means certain, are: *Oxalis Noronhæ*, *Combretum rupicolum*, *Erythrina aurantiaca*, *Cereus insularis*, *Bumelia fragrans*, *Solanum botryophorum*, *Pisonia Darwinii*, and *Ficus Noronhæ*. Of course, there is absolutely nothing in the composition of the vegetation to suggest an African origin, and Dr. Ihering may have intended Tristan d' Acunha when writing Fernando Noronha. The exceedingly meagre vegetation of South Trinidad (20° 30′ S. lat., 29° 22′ W.) includes *Asplenium compressum*, a fern only known elsewhere from St. Helena; and the genus *Achyrocline* (Compositæ), which is both African and American, is represented by an endemic species. It may be mentioned incidentally that Dr. Ihering enters somewhat fully (42) into the origin of the southern insular floras; and he also discusses the possible and probable agents of dispersal, arriving at much the same conclusions as the writer, though he does not appear to have known of the existence of the reports on the botany of the *Challenger* Expedition. Turning to Ascension Island, it was not to be expected that after the investigation of such keen botanists as Hooker, Burchell, and others, any species had escaped detection, yet an American expedition adds three proposed new species (43), namely, *Rubus nanus*, *Asplenium ascensionis*, and *Nephrodium viscidum*. The author was manifestly unaware of the extent to which plants were introduced into the islands from Kew and other places, during the period that cultivation was attempted, for the purpose of supplying ships with vegetables, and even for more ambitious schemes, such as Cinchona planting. There is hardly a doubt that the *Rubus* is a descendant of an introduced species. This view is confirmed by the statement that "it appears to be a very distinct and pecular species of a genus not otherwise represented in the flora of the islands of the South Atlantic". There is a fragment of the *Asplenium* in the Kew Herbarium, collected by Don on his journey to

Sierra Leone; and the material of the *Nephrodium* was hardly sufficient to base a new species upon. Mr. Watson expresses regret that the opportunities of the expedition for exploring the island were not greater, "because they might have considerably increased the number of indigenous species known"; but, of course, this is most improbable, as the island has been as thoroughly explored as any island in the world.

Concerning the very small remnant of the original vegetation of St. Helena, no further information has been recorded during the last decade. It may be mentioned, however, that Penhallow's "Flora of St. Helen's Island" has, by some oversight, been confused with St. Helena in the Atlantic (44).

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FOLDS AND FAULTING: A REVIEW.

PART I.

TO those who are interested in the deep and complex problems geology is called upon to solve, the triennial geological congresses afford a special means of obtaining fresh light and knowledge, under the guidance of leaders whose life-work has been carried out in the country, the structure of which they are called upon to explain. Men holding the most varying views are brought to examine the same facts from their different standpoints, and the discussions which result from their contact lead to conclusions of permanent value and wide-spreading influence.

The meeting of the latest congress at Zurich, the subsequent traverses across the Alps, and the publication of the Livret-guide explanatory of these excursions, have directed the minds of a large number of geologists to those movements of the earth's crust, which have given rise to that splendid range of mountains, whose detailed structure and scenery we were called upon to examine.

In the earlier part of the century the most varied theories had been enunciated in regard to these inequalities of the earth's surface, the facts of mountain structure being little understood. Volcanic agency was made largely responsible for these upheavals ; the wedge-like intrusion of igneous masses, the liftings of an internal igneous nucleus, or the sinking of the ground by the removal of volcanic material, being all called into requisition to account for the phenomena. Simple upward pressure stood in sharp contrast to horizontal or lateral compression, and around these rival standpoints raged a combat not less severe than those which in later years have divided the geological world.

The conceptions which have in the last decade gained the ascendency in all probability first had their rise in the

masterly work of the Brothers Rogers, when studying the Appalachian range. The full results of their labours were brought before the British public by Professor H. D. Rogers in 1857 (1), and it is interesting therein to note the great advance already made towards a true knowledge of the elements composing a mountain range. These observers early recognised that every tract of the earth's crust which had undergone upheaval had a wave-like form, and that faults were merely separated, disarranged portions of what were originally continuous undulations; more than this—that in regions of great disturbance the strata were arranged in belts of parallel waves, the region of maximum disturbance being marked by a closer folding of the same. Each primary undulation itself might be thrown into secondary waves, which, though not necessarily parallel to the primaries, nevertheless preserved a constant parallelism between themselves, and a third class might also be produced, giving rise to the so-called rolls of strata. But these were not merely simple waves of undulation, for the folding changed its character from *symmetrical* flexures, dipping equally on both sides, to *normal* flexures, where the dip was steeper on one side than the other. Finally, in regions of greatest disturbance the flexures are overfolded, there being an actual inversion, or doubling under, of the steeper side of each curve. Thus the lines bisecting the half curve, the axis-planes, usually dip at a very low angle, and towards the region of maximum disturbance.

Further, they recognised that a close series of plications may appear as one of simply conformable deposits, especially in cases where slaty cleavage is set up. Failure to recognise this principle has in subsequent years given rise to many difficulties, and complicated theories of migration have been invented to account for the repetition of zones of the same fossil at different levels in the same group of strata.

Unlike Elie de Beaumont, they came to the conclusion that crust waves were not straight only, but also curvilinear, and that flexures grade away from the districts of maximum disturbance and contortion, being highly plicated in those

regions, and gradually opened and flattened out as they recede from such a centre.

Whilst noticing cases of more ordinary transverse faulting, they laid special stress on the occurrence of the greater longitudinal overthrusts parallel to the main anticlinal axes, which, owing to more or less compression, had snapped and given way in the act of curving. It was found in South-west Virginia that the youngest stratum first disappeared, the older ones following in succession, until finally the Lower Limestone of Cambrian or Lower Silurian age comes to absolutely overlie the Carboniferous Limestone. The strata thus engulfed represent a thickness of over 8000 feet, and the uninverted side of the wave had been driven eighty miles over the more inverted portion.

Those who are acquainted with the present condition of this discussion will at once appreciate the solid basis upon which the above conclusions were founded, and it is equally to the credit of Professor Rogers that he came over to Europe to test the accuracy of his conclusions in those regions where similar results might be expected. The Devonian strata of the Rhine, as also the Jura range, again showed the passage from broad waving of the strata to that of more complicated flexuring, but, above all, in the Alps he found the absolute comparison he was in search of. He says: "We behold an exact counterpart, in the stratification or structure of a single flank of the Alps, of that folding with inversion, which characterises the Appalachian chain, or that of the Ardennes, a single side of the Alps being the equivalent of the whole of either of these zones; it consists, that is to say, of an undulation in one direction". He concluded that the flexure could only be due to the actual pulsation of fluid matter beneath the crust, propagated in the manner of great waves of translation from enormous ruptures occasioned by the tension of elastic matter. The various features observed were ascribed to the combination of an undulating and a tangential movement, accompanied by an injection of igneous veins and dykes into the fissures occasioned by the bending.

Sir Henry de la Beche (discussing Professor Rogers'

hypothesis) (2) pointed out that the conditions observed were perfectly compatible with the assumption that lateral pressure, *viz.*, that of masses of the earth's crust against other masses along lines of fracture on the surface, had been the cause of these phenomena. About the same time, Sir R. Murchison (3) had clearly seen that, in those parts of the Alps where crystalline overlay unaltered sedimentary strata, this result would be due to "one enormous overthrow, so that over the wide horizontal area, the uppermost strata, which might have been lying in troughs or depressions due to some grand early plication, were covered by the lateral extension over them of older and more crystalline masses".

In 1856 Professor Nicol (4) hinted at the possibility of such conditions having likewise existed in the Scottish Highlands. "The termination of the Quartzite period seems again to have been marked by convulsions. To these we must refer the action by which the higher portions were converted into gneiss, or *this gneiss* if a pre-existing rock forced over the quartzite."

Two years later, Sir R. Murchison (5), examining the same country, was led to a conclusion which for twenty-five years proved a stumbling-block to the larger number of English geologists. To him the succession in North Scotland appeared as a perfectly simple upwardly ascending series, the upper quartzite and upper gneiss being not only apparently, but in reality, younger than the Durness limestone they seemed to overlie. To this view the most distinguished amongst our English geologists rallied, and Professor Nicol's protest in 1861 (6) carried no conviction to the minds of the majority. Undoubtedly he weakened his position by doubting the existence of gneissose rocks above the limestone series, and assuming that simple faulting might account for all the phenomena observed. Nevertheless, in justice it must be allowed that he recognised that "a comparatively very small amount of inversion and extrusion of older crystalline masses will suffice to explain any of the Scottish sections, even as drawn and described by the advocates of an overlying younger gneiss". After

giving an example of inversion and intrusion, he adds: "Until some rational theory is produced of the mode in which an overlying formation, hundreds of square miles in extent, and thousands of feet in thickness, can have been metamorphosed, whilst the underlying formation, of equal thickness, and scarcely less in extent, has escaped, we shall be justified in admitting inversions and extrusion equal to those in the Alps".

If somewhat vaguely expressed, it is evident that Nicol's thoughts were already tending in the direction of a truer solution, but the answer given by Murchison and Geikie (7) appeared so conclusive and overwhelming that further discussion was silenced. Pure stratigraphy had triumphed, and tectonic geology slumbered, in so far as the British Islands were concerned. Meanwhile, on the Continent and in America, materials were being accumulated, destined eventually to lead to conclusions of the highest importance; but before the new views could be brought into prominence, one of the conflicting hypotheses had first to gain a commanding position. Only with the abandonment of Von Buch's hypothesis of elevation by Plutonic upheaval, and the recognition that Elie de Beaumont's brilliant theories of mountain structure were untenable, could lateral pressure due to contraction of the earth's surface become a serious factor in the discussion of foldings. Hall, De la Beche, Prevost, Pratt and Fisher had been largely instrumental in bringing about this valuable result, whilst Daubrée had attacked the question from a practical standpoint, and had carried out a large number of experiments having a most important bearing on the point at issue. In 1875 Dana, adopting a line of thought parallel to that already pursued by Babbage and Herschel, pointed out the important relation existing between mountain formation and the previous deposition of thick layers of sedimentary deposits, the position of the future mountain range being first marked out by the slow formation of a geo-synclinal, the deposition of sediment being concurrent with the progress of depression. As the geo-synclinal descends, a geo-anticlinal must be formed on one side, or possibly on both, but in the Appala-

chians the first condition seems alone to prevail, the lateral pressure having acted unequally from the oceanic and from the continental sides.

When a vast thickness of strata has thus been depressed below the surface, the bottom of the geo-synclinal eventually becomes weakened by the heat rising from below, and, partially yielding to the pressure, the rocks become displaced, upturned, folded, and fractured.

In the same year Suess published his volume, *Die Entstehung der Alpen*, which was destined to at once raise the whole discussion to a higher level than it had ever before attained. The labours of Favre and Lory in Switzerland, Beyrich in Bohemia, Abich in the Caucasus, Stoliczky and Medlicott in India, Von Richthofen in the Carpathians and China, not to mention a number of other observers, were laid under contribution to assist in building up those generalisations, the results of which we enjoy to-day.

Accepting as his theoretical basis the contraction of the earth's surface, Suess pointed out that the Alps formed part of one enormous belt encircling the globe, and were therefore not merely local occurrences, but component parts of one vast movement. Further, that not merely the central crestal ridges should form the subject of discussion, but that a whole region or series must be taken into account. As a result of his observations, he is of opinion that there are directions of mountain flow, the forward movement being marked by the overlapping or overthrust of beds belonging to the Alpine system, over those which have not shared in the movements resulting from contraction; whilst, in the rear of the mountain-axis, great faults and considerable fractures are produced, these, as in the Apennines and Carpathians, occasionally giving rise to important lines of volcanic eruption.

Not only, therefore, are many of the most magnificent mountain systems asymmetrical, but they have also a definite movement, flowing towards, or from, the poles. From the Cordilleras to the Caucasus this flow is uniformly northwards; whilst across the whole of Asia the conditions,

whether stratigraphical or tectonic, are reversed, the principal massifs forming a curve, presenting their convex front towards the south. On this hypothesis, therefore, mountain ranges result from unequal contraction, and are not the products of forces acting equally in opposite directions.

But further, the contracting mass is affected by hindrances, and re-acts upon these in a peculiar and striking manner. After overlapping the old gneiss of the Dôle area and the ancient continental regions of the Black Forest and Bohemia, the younger Alpine series, freed from the resistance of these higher districts, spread over the lower grounds of Galicia, unconformably overlying the older carboniferous beds of that province; Professor Suess, indeed, considers that the north-east and south-west foldings, which can be traced through the Erzgebirge, along the Rhine into Belgium, and over to our own islands, are the result of the action of that powerful pressure which the outstanding sentinels had only apparently successfully resisted.

The discussion as to the transgression of the Cenomanian, both in Europe and Asia, has proved that these earth folds have not been mere local phenomena, but that we are dealing with events of the highest importance in physical geology. It has now been shown that the Upper Cretaceous beds transgress over the older in Northern Europe, Northern Africa, India, and Eastern Asia, and we have also lately learnt that the same holds good for Chili.

At the recent Swiss Congress Professor Steinmann gave an account of the fauna of the Quiriquina beds in that country, and showed that three-quarters of the Ammonites there met with are absolutely identical with Indian types, the Gasteropoda and Lamellibranchiata being also similar to those occurring in the Utatur strata, near Pondicherry, and therefore he deduces that, like these, the Chilian beds must be of Cenomanian age. The similarity extends yet further, the transgression being as strongly marked in these strata as in the Indian type.

The Shore Cordilleras consist of mica schists, granites, etc., and these older rocks are directly overlaid by the Quiri-

quina beds, the Jurassic strata being entirely absent at this point, though present to the north and south. The conclusion forced upon us therefore is, that here we have a remnant of an old continent, or land barrier, which remained when the great depression took place, whose effects are so strongly marked throughout the world.

Whilst the publication of this work by Professor Suess, with its ingenious blending of ascertained fact and well-balanced hypothesis, opened up a new world of activity in geological thought, the labours of Escher, Baltzer, and Heim were laying the foundations of a new structure which was to celebrate the apotheosis of the fold. Commenced by Baltzer in 1873 with the issue of his paper on the Glärnisch, it was consummated by Heim in 1878 in the publication of the *Mechanismus der Gebirgsbildung*, with its wealth of detail and beauty of illustration, which gave a new impulse to geotectonic discussion, and influenced deeply many of those geologists who are now taking a front rank in this particular branch of inquiry.

Professor Heim lays special stress on the importance of foldings in mountain structure. His theory may thus be succinctly set forth. At a certain depth beneath the earth's surface the rocks are loaded far above their power of remaining solid. This pressure is applied in all directions, so that each individual component is equally affected, and even the most massive rocks are maintained in a state of latent plasticity. Should there now ensue a disturbance of equilibrium through the application of a new force, the horizontal mountain-forming compression, there will be then a mechanical transformation in the deeper-seated portions without fracture, but nearer the surface in the more massive materials fracture would result. It follows, therefore, from this that all the foldings we see in the Alps have been formed deep in the earth's crust, and the whole of the strata that overlay them have disappeared under the action of denuding and erosive influences. Ordinary faulting is of the rarest occurrence, and the throw rarely exceeds a few metres, and even then the effect is purely superficial. He points out as a matter of fact that in the younger geo-

logical strata transformation with faulting predominates; whereas, in the older, transformation without fracture becomes more and more common, and gives as a further result the fact that the outer chains consist of Mesozoic rocks with beautiful gentle curvatures, whereas the innermost most ancient ones are much more sharply and irregularly bent.

This, then, is his broad principle: Where rocks have been gradually depressed under a mass of sedimentary strata they will pass more or less readily into that condition suitable for the development of pure folding. Supposing, therefore, 3000 feet of superincumbent strata should suffice to induce the necessary change in clayey materials, it would require a far greater weight to render massive limestones sufficiently plastic to obtain the like result.

In an eloquent chapter (Theil ii., pp. 114-128) Professor Heim attacks a position which had already been proved untenable, and by showing the eruptive rocks of the Alps to be in every case older than the main upheaval, he has effectually dispelled the reasoning which would attribute to them the cause of mountain formation. If there be any who, on the basis of Lawson's study of the gneiss in the Rainy Lake region, regard most of these as of very late intrusion, they have before them a most difficult task should they attempt to apply their theory to the whole of the Alpine gneisses.

The conclusion arrived at by Professor Heim respecting the Alps is decided and clear. He refuses to admit that they have been elevated by eruptive agency; the rocks are older, and have been brought to their present position in a passive manner, and he maintains that eruptive rocks do not produce a mountain chain. He affirms that everything at present known as to the structure of the crystalline rocks composing the Centralmassif agrees absolutely with the conception that they are the arched portion of a fold system of the crystalline earth crust. The crystalline rocks are frequently bent in such a manner at their point of junction with the sedimentaries that they agree more or less closely with them as regards position. The sedimentary rocks,

often in their condition very similar to the crystalline, not only skirt the Centralmassif, but often penetrate it, and form an integral portion of its construction. These are not separated portions embedded, but the remains of closely compressed troughs. The Centralmassifs have not folded the sedimentary chains by their lateral activity, but are themselves zones of the earth's crust which were formerly overlaid by sediments, have since undergone compression, and are now exposed through denudation and erosion. The folding of the Centralmassif is of younger tertiary age, and therefore synchronous with that of the sedimentaries ; any older flexuring could, at most, have been of a very feeble character (*loc. cit.*, p. 178). All the Centralmassifs and all the limestone chains of the Alps have been formed, because the sum total of their foldings represents the force necessary to neutralise a definite tangential compression. The innumerable magnificent ridges and peaks, so varied in form and outline, are the outcome of the same activity and of the same period, no matter whatever may be their materials; they are probably the result of a pressure able to overcome the cohesion of the rocks and producing folds, whereas the remainder of the earth's crust had not shrunk one-hundredth part towards its centre (*loc. cit.*, p. 186).

Having thus established the principle that folding is the main and necessary result of the compression, he proceeds to a study of the folds themselves, these being either normal, with the beds dipping away from the axis plane in the arch, and towards it in the trough ; isoclinal, where both the anticlinals and synclinals are in such a position that the beds are parallel to the axis planes and appear to be concordant in stratification ; and fan-shaped when the strata both in the anticlines and synclines dip towards the axis planes, producing by this means the appearance of the younger rocks underlying the older. Should this latter form become very oblique, either a lying isoclinal overfold or fan-shaped one may be produced, the middle limb of which may be squeezed out. These may pass directly into fold faults, there being no strict boundary between the latter and the overthrust planes.

The Alps consist of about a dozen main folds, but the total number of secondary ones cannot be estimated. Nevertheless, taking the whole of the great movements into consideration, the earth has been reduced by no more than a hundredth of its previous circumference. Pressure may have already commenced its action in Cretaceous or Eocene times, the highest existing fold being in all probability the earliest, and at the time that the outermost post-Miocene Alpine chains were being formed, the inner zones were already undergoing denudation and erosion. Seeing, however, that the Central Alps are higher than the outer ridges, it follows that elevation must have been more rapid than denudation.

We must now call attention to a special case involving the highest conception of a fold as yet submitted to the notice of geologists. We refer to that of the Great Glarner Doppelfalte or Doublefold. Commencing near the Central-massif, west of the Reuss, it continues as far as Ragatz, the northern part of the fold having a length of ninety kilometres, and the southern, one of forty-eight. In the whole of this region, on both sides of a central axis, older beds of Verrucano overlie the younger Eocene, the two being separated by a limestone complex, the Lochseitin Kalk, in which Escher von der Linth, after much wearisome research, found Jurassic Belemnites and Ammonites. The theory propounded supposes that at this point intense folding had taken place in two opposite directions ; the arch has moved forward, whilst the trough has travelled in the opposite direction. The septum, or middle limb, being drawn out owing to the advance of the arch, has at the same time been squeezed out between the arch and trough cores. In the above case, through this movement the whole series from the Permian to the Upper Jurassic is only represented by a highly altered limestone a few metres in thickness.

The above-named two trough limbs are connected deep beneath the surface, and the Eocene core being compressed between the two foldings has itself become enormously contorted and bent. The theory of plasticity already referred

to excludes any supposition of overthrust faulting, and in consequence we are forced to the conclusion that these are really enormous overlying folds travelling in opposite directions. We would lay special stress on the theory here enunciated (see fig. 1), because presently we shall see that exception has been taken to the views here set forth.

The publication of Heim's valuable work, marked alike by beauty of illustration and breadth of theory, undoubtedly gave a great impetus to the study of these tectonic questions, but in justice it should not be forgotten that part of any credit due to a juster appreciation of mountain foldings must be given to Professor Baltzer, who, by his work on the Glärnisch, and subsequently for the Swiss geological

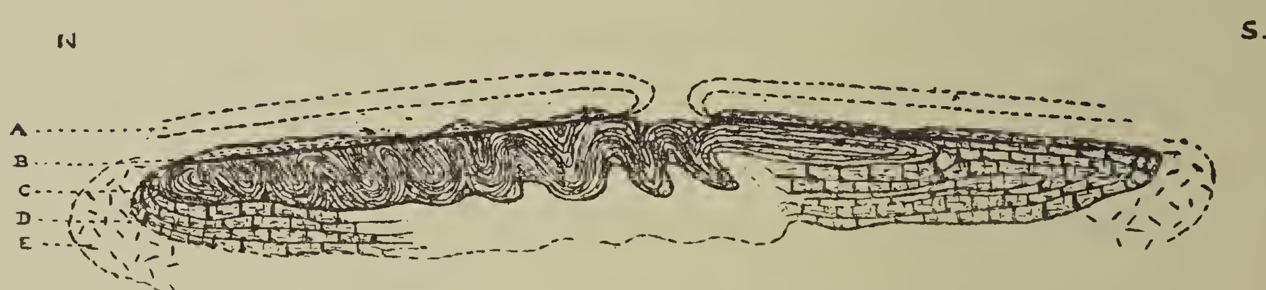

FIG. 1.—Illustrating Heim's theory of the Doppelfalte.

A. Arch Limb (Mesozoic rocks mainly), now denuded away.

B. Arch Core (Verrucano), still capping the higher summits. The dark line represents the (middle limb) (squeezed out) between the arch and trough cores, all the rocks from Permian to Jurassic being reduced to the Lochseitin Kalk.

C. Trough Core (highly contorted Eocene shales).

D and E. Trough Limb (Mesozoic rocks and Verrucano).

The same conditions are repeated for the Southern Fold.

map, has done much in support of the position taken up by Heim.

In two countries especially the influence of the new views early made itself felt: in Scotland, by throwing fresh light on the complicated questions which had arisen, and in Belgium, enabling Köhler to attack the difficult problems connected with the Westphalian Coalfields (8), wherein he explains these overthrusts as being due to the overturn of folds, on the same theoretical basis as that adopted by Heim for the Doppelfalte. The Scottish discussion, after its long period of stagnation, was now re-opened, first by Drs.

Hicks and Callaway, but principally by the work of Professor Lapworth, who, in a series of papers on the Girvan succession, and the "Secrets of the Highlands," showed the marked influence Heim's work had had upon his own views, the publication of these articles having also the effect of inaugurating the application of the new principles to the problems and difficulties connected with the tectonic structure of our own island. What, then, were the results of these contributions from a tectonic point of view?

Undoubtedly, the principal results were: The recognition of the importance of isoclinal folding in the Ayrshire Palæozoic rocks; the discovery that zones, when carefully traced out, showed the actual presence of enormous folds; and that the repetition of the same fauna in upwardly ascending series was the result of the close parallel packing of a group of anticlinal and synclinal folds. The outcome of these labours has also served to overthrow the complicated conception of "Colonies" introduced by Barrande to account for the peculiarities observed in the palæontological succession.

Having thus far successfully endeavoured to elucidate these varied and difficult problems, Professor Lapworth next turned his attention to mountain structure, and was led to the conclusion that folding was the result of lateral compression. He observes: "At the foot of a mountain range the inward thrust and the outward counter-thrust are approximately equal in amount and opposite in direction, and the resulting folds are normal and regular (normal or amphiplexal folds), but as we proceed towards the centre of the range, whilst the thrust inwards remains approximately the same, the counter-thrust outwards is aided by the effect of the gravity of the mass above, and these two unequal forces are applied to the stratum obliquely with respect to each other. As a natural consequence, the axes of the rock folds no longer remain vertical, but *slope obliquely outwards, i.e.*, in that special direction in which the folding and ascending strata encountered the least resistance to their extension" (*Geol. Mag.*, p. 198).

In the discussion on overfolding, Professor Lapworth practically recognised every stage; from the condition where no separation of arch limb, middle limb, and trough limb had taken place, and where the middle limb has been rolled out between the arch core and trough core (this being Heim's theory for the Doppelfalte), to that wherein dislocation actually ensues; in which case the arch portion travels in a rigid mass over the trough. It will therefore be seen that Lapworth practically adopted Heim's conclusions, with all their far-reaching hypotheses.

Applying these to the Sutherland area, he concluded that, far from dealing with a simple succession, the Upper Gneiss represented only a part of the Sutherland gneisses that had undergone one of these tremendous overthrusts, the rocks taking part in the movement having suffered great deformation and contortion, thus giving rise to those sheared and pseudo-foliated structures, which he has included under the term "Mylonitic".

Almost simultaneously Dr. Callaway came to a similar conclusion, whilst Dr. Hicks announced that the highest beds were, in fact, parts of the oldest rocks occurring in a broken anticlinal, and Professor Bonney likewise showed that the coarsest gneiss had been crushed into a perfectly schistose rock.

Powerful as were the facts and arguments brought forward by these various observers in weakening the position of the Murchisonian hypothesis, its final overthrow was only achieved by the publication of Sir Archibald Geikie's letter, and the first summary on the survey work of the Durness-Eriboll area (9), and thus, by the frank adhesion of the Geological Survey, was closed the excited controversy, the views as originally propounded by Professor Nicol, and rejected during so many years, being to a great extent triumphantly vindicated.

It was now recognised that these Scottish rocks, first commencing as gentle foldings, steepened gradually on the western front until, bending over, they became disrupted, and the eastern limb pushed forward. Reversed faultings

still further complicated the discussion, but special stress was laid on the thrust planes, which were regarded as reversed faults of very low hade. It is along such planes that the rocks in Durness have been thrust a distance of over ten miles. Here had taken place the re-arrangement of the mineral particles, giving rise to the crystalline schists already referred to, whilst at the same time the vertical worm tubes in the quartzites underlying the thrust plane have been flattened, drawn out, and bent over in a direction perpendicular to the strike of the same (*Nature*, 1884, pp. 29-35).

The final result of the great contest was hailed on all hands with the liveliest satisfaction. Professors Lapworth, in the *Geological Magazine*, Bonney, at the Geological Society, and Judd, in his presidential address before the British Association, 1885 (wherein he specially recalls the important services rendered by Nicol in the controversy), all recognised the importance of the new departure. The first two of these observers had already taken part in the fray, whilst the latter, after a visit to the ground, recognised the truth of Nicol's position, and in 1877 had urged him to re-open the whole question, but unavailingly.

Younger students can now follow with interest the varied phases and details of this battle of giants, and, whilst learning many a valuable lesson from their failures and defeats, may profit by their discoveries and their triumphs.

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(*To be continued.*)

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VARIETIES OF LEUCOCYTES.

PERHAPS recently more than ever has minute attention been given to the structure and behaviour of wandering or free cells, and many of the problems offered by the leucocyte have become familiar to every anatomist and physiologist. It may be useful to attempt a succinct description of some of the recent facts elicited, and of the views enunciated by various authorities.

By "leucocyte" it is now customary to mean any vagrant cell of the organism, not merely a cell whose habitat lies in blood and lymph, but also the cells which haunt the serous (cœlomic) chambers and the intercellular interstices of the tissues. It was at one time tacitly assumed that the wandering cells in each and all of these localities were actually identical (1). It is now well recognised that between the wandering cells in various localities considerable and specific differences exist; and that even in one and the same locality a mixture of wandering cells distinctly differing one from another in morphological characters co-exists, although at the same time each of the various cell-forms appears to possess a region of distribution more or less proper to itself, a habitat of its own.

The varieties of leucocytes now generally recognised to be separable are the following (2):—

I. The lymphocyte or small hyaline cell.

II. The large hyaline cell.

III. The finely granular oxyphil cell.
IV. The coarsely granular oxyphil cell.
V. The finely granular basophil cell.
VI. The coarsely granular basophil cell.

The following is a summary of the characters made out for each of these several varieties.

I. *The Small Hyaline Cell.*

About the same size as a human red blood corpuscle, this cell is often termed "lymphocyte" because so numerous in lymphoid tissue, *e.g.*, lymph glands, tonsils, etc. Its scanty cell-substance appears quite devoid of granulation. Its nucleus is spheroidal and deeply tingible. The cell does not exhibit amœboid movements, nor does it readily adhere to surfaces as do many of the wandering cells. It does not appear to have the power of ingesting particles, and therefore cannot be included among "phagocytic" cells. The birthplace of this cell lies undoubtedly in the lymphoid tissues, probably in lymphoid tissue of all kinds; the production of it takes place especially at the "germ-centres" of the lymphatic tissue where mitotic figures are always abundant (3); and in the lymph issuing from the active tissue young cells abound. The small hyaline leucocyte is probably to be considered in all cases as a young cell—an immature form. Into the blood it finds its way almost entirely *via* thoracic duct. In the blood it is not the most numerous form of leucocyte; its numbers there are subject to phasic variation, becoming highest two to three hours after the ingestion of a full meal, and then forming even as much as a third of all the wandering hæmic cells, and numbering about 2500 lymphocytes in the cubic millimetre of blood. In lymph from the thoracic duct it is much more numerous absolutely as well as relatively to other leucocytes; there not uncommonly 25,000 lymphocytes per mm^3 of lymph can be estimated, and they form more than 90 per cent. of all the cells in the lymph. In "lymphatic leukœmia," a diseased condition characterised anatomically by enlargement of the lymphatic glands, the normal number

of small hyaline leucocytes in the blood is usually very greatly exceeded.

II. *The Large Hyaline Cell.*

A spheroidal cell with a rounded, often reniform, nucleus. The nuclear network consists of very fine chromatin threads enclosing relatively wide meshes; as a whole, therefore, the nucleus does not usually take on any great depth of stain. So long as the cell remains alive the cell-body is devoid of visible granularity, and is apparently homogeneous, but when the cell is dead the cell-body can be distinctly tinted by methylene blue and similar colouring matters. Examination under high magnifying powers then resolves what under lower appears to be a sheet of evenly stained substance into a cloud of minute and feebly stained particles embedded in a matrix completely unstained (2). These tiny amorphous and shrunken particles are all that are demonstrable in the way of cell contents, and are the homologues of the obvious granules in granulate cells, *e.g.*, secreting cells, many leucocytes, etc.

The large hyaline leucocyte is usually not amœboid; under many circumstances it proves itself to be nevertheless a capable phagocyte; that is to say, it can take up particles from its environment and enclose them within its cell-substance. Whether a cell in order to ingest particles must be amœboid is so far as I know undetermined, but it is conceivable that it need not be. Certainly a cell to be free and "wandering" need not be amœboid; in its "wandering" it may be the passive subject of outside forces acting upon it.

The large hyaline cell occurs in blood, in lymph, and in the tissue-spaces; it is most common in the latter. Normally it forms less than 10 per cent. of all leucocytes in the blood. When its numbers in the blood are large the blood is usually of low specific gravity, of low hæmoglobin value, and poor in chromocytes; thus it is numerous in the anæmia ensuing upon typhoid fever (4); it is also numerous in the anæmia of pregnancy. In the latter condition, though hardly in the former, there is reason to

believe the poverty of chromocytes is apparent rather than real, and that the blood is rich in plasma rather than poor in red corpuscles, *i.e.*, is polyplasmic. The plasma itself is under these circumstances not deficient in solids. In lymphatic leukœmia, where lymphocytes are in excess in the blood, the large hyaline cells are usually quite scanty.

III. *The Finely Granular Oxyphil Cell.*

Wharton Jones was the first (in 1841) to draw attention to the *granules* existing within leucocytes (5). After establishment of the view that a living animal cell consists structurally of a spongy reticulum of protoplasm containing less active material, paraplasm, in its meshes, Heidenhain and Langley early demonstrated and worked out the details of the periodic granularity of secreting cells (6). They showed that in such cells activity is commonly associated with more or less continuous storage, and with more or less intermittent discharge of material elaborated in the form of granules by the living network of the cell-body. Among the followers of these investigators have been Ehrlich (7) and, later, Altmann (8). The latter has passed from somewhat slender premises to the speculation that a discrete granule is the elementary unit of all biological structure, and that every cell is a colony of such "elementar organismen," just as the body of a metazoon is a colony of cells. These elemental bioplasts, if aggregated into cells, he terms cytoblasts; if free, the bioplasts are autoblasts; micrococci, it is urged, are autoblasts, the granules of the leucocyte, cytoblasts. But Altmann has not added much solid fact to the subject. On the other hand Ehrlich has added considerably. The latter has elaborated a scheme of microchemical tests for the granules of the various cells observed. According to Ehrlich, the staining solutions used in histology may be considered in two groups: (1) acid solutions, (2) basic solutions. In "acid" solutions the staining principle is the acid although the dye may be a chemically neutral salt; tinctorially it reacts as a free acid. Ehrlich's scheme of examination rests on determining whether a cell-granule can be stained by acid colours more

readily than by basic, or *vice versâ*. Thus, ordinary fuchsin is hydrochlorate of rosaniline; in this compound it is the base, the rosaniline, which is the staining principle, the acid, hydrochloric acid, is not the staining principle; the staining solution is therefore in Ehrlich's sense a basic one (9). Again, picrate of ammonium is an "acid" dye, because in it the picric acid is the staining principle. Ehrlich terms the cell-granules which can be more readily tinged with acid dyes, "oxyphil granules"; those which can be more readily tinged with basic dyes, "basophil".

The technique introduced by Ehrlich and his pupils in their researches of this kind is reliable and capable of wide application. In its employment it is, however, necessary to remember that it is not the living cells which are usually dealt with, and that in the course of fixation the cells and their granules become modified. It is therefore necessary to adopt some particular mode of fixation and of application of the stain as a standard, and thus to obtain standard oxyphil and basophil reactions. Kanthack and Hardy (2) proceed as follows: thin films of blood or lymph upon cover-glasses are dried in the air, and then passed quickly three times through a Bunsen flame. The films are next placed for 30″ in a solution of 5 grm. eosin in 100 c. cent. of 70 per cent. alcohol. The excess of eosin is then removed by placing the film in water, after which the film is again dried, passed thrice through the flame, and finally counterstained with Löffler's methylene blue solution (basic stain).

Examined in this way there are found among wandering cells two kinds with oxyphil granules, and two kinds with basophil granules. The cell with small oxyphil granules is that which I have placed first on our list. It may be considered κατ' ἐξοχήν the wandering cell of mammalian blood. The cell is of medium size, about 10 μ; its cell-substance contains small granules slightly more refracting than the ground substance in which they lie; the granules are particularly obvious in the rabbit. The nucleus is peculiar in form and fairly characteristic as distinguishing the cell from other leucocytes. It is multipartite and of extremely

irregular outline, consisting usually of lobes linked together by thread-like bonds which are composed almost entirely of chromatin. It was at one time a question whether this nucleus was a sign of degeneration and breaking down of the cell,—was in fact a "fragmentation nucleus,"—or whether it was due to some form of reproduction by budding taking place. Neither of these suppositions is now accepted; the diversity of shape of the nucleus is almost certainly attributable to distortions produced in it by the extreme amœboid activity of the cell-body (35).

The cell is a vigorously amœboid one. It is also a phagocyte. Halliburton and Brodie find that solutions of certain nucleo-albumins kill and break up these leucocytes very speedily, although without effect upon the activity of ciliated cells from epithelia (63). There is of course much fluid in the composition of these mobile cells, but the granules embedded in them never exhibit Brownian movement so long as the cell is healthy, or even so long as it is alive (10). At death of the cell its granules are at once given over to Brownian movement and to such a degree as to produce in the whole cell a shimmering appearance. This phenomenon can be easily studied in many cells of *pus*, which are often for the most part dead leucocytes. In pus cells also irregular nuclei can be studied which are really fragmented, sometimes curiously regularly into rosette forms (10); a comparison of these forms of degenerated nuclei with the irregular-shaped nuclei of the active cells is instructive as to the great real difference between the two, though both are multipartite. The granules in this cell have been described by Ehrlich and his pupils as neutrophil, under the impression that they were not stained readily by "acid" dyes. Kanthack and Hardy show that the granules are really oxyphil. They point out that in some rodents the granule is especially oxyphil, a fact that can be easily confirmed by any one who will take the trouble to stain with a standard eosin solution a dried film of rabbit's blood. The granulation in these cells becomes particularly obvious and particularly oxyphil in animals recovering from, or just recovered from,

acute bacterial infections of various kinds (11). The cell has a precise and limited distribution in the body, for under normal circumstances it occurs only in the blood, there constituting usually about 75 per cent. of all the leucocytes. It is certain that it has been detected multiplying by karyomitosis while still within the circulation and under normal conditions; it may therefore pass its whole existence within the blood-vessels; yet specimens of it showing karyomitosis are very uncommon (12). Some hold that it undergoes direct division more frequently (13). It is essentially a blood-cell, and is present in fœtal blood before the lymphocyte appears (14).

IV. *The Coarsely Granular Oxyphil Cell.*

This cell is large, usually above 12 μ diameter. Its nucleus is of less irregular shape than that of the finely granular oxyphil cell; very often its nucleus is reniform, or in the form of a horseshoe. The granules contained in the cell-body are large, shining and highly refractive. In shape they vary in different animal species; most often they are spherical, but in the cat are ovoids, and in the horse cuboids (10). They are largest and least numerous in the cells of the horse, amounting generally to not more than a dozen, whereas in many animals one cell may contain a hundred of them. They are remarkably readily and deeply stained by "acid" dyes, perhaps most readily in human blood, and least readily in rat's blood. When to fresh undried rabbit's blood a little Ehrlich-Biondi fluid is added the granules in the still living cell take up the acid fuchsin of the mixture. Treated with osmic peroxide the granules turn brown almost as deeply as do fat particles, but they are not soluble in alcohol or ether. In their high refraction and oxyphil affinity they resemble the red blood corpuscles, and like them contain both phosphorus (10) and iron (15); but they are quite colourless. Some observers from their microchemical reactions have concluded that the granules are proteid (16); if so the proteid may be nucleo-albumin; but these cells do not appear to play a part in the clotting of the blood or lymph. The granules may be considered

to be nutritive material accumulated within the cell, or to be substance elaborated by the protoplasm in virtue of secretory activity. Ranvier (17) takes the former view, Ehrlich the latter. Kanthack and Hardy (2) have shown that by bringing into the proximity of the cell certain bacteria a condition of extreme activity can be produced in it, and that under this excitation the body of the cell rapidly becomes clear of its granules. The granules are after a while reformed, but the new ones are at first not quite like those previously present. This observation of the lysis of the granules is fraught with great interest, because the same observers have shown that the cell is able to greatly damage the bacteria offered to it, and this it must effect by means of its secretion. The cell may therefore be looked upon as a free unicellular gland.

A further peculiarity of the cell is that though it is very vigorously amœboid, and can throw out remarkably active pseudopodia, it is incapable of incepting particles, and vacuoles are never seen within it. By adding to fluids containing these and other leucocytes particles suitable for recognition, *e.g.*, pigment, bacteria, crystals, etc., the large hyaline and the finely granular oxyphil cells take these up speedily, imprison them in vacuoles, and, in the case of proteid particles and bacteria, in many instances digest them; but the coarsely granular oxyphil cells, although they may affect contact with the particles, have never under any circumstances been seen to incept any. This, presumably, is the reason why in the blood of malarial patients the hyaline and finely granular leucocytes are pigmented, but the coarsely granular never exhibit pigmentation. The formation of vacuoles in the coarsely granular oxyphil leucocyte, if it occurs at all, is an event of great rarity.

The habitat of the coarsely granular oxyphil leucocyte includes the blood, the lymph, serous fluids, and the clefts of the connective tissues, except cornea and tendon. They are relatively to other leucocytes more numerous in the peritoneal fluid than in the blood or lymph; in the blood of the cat they constitute from 2 per cent. to 8 per cent. of all the leucocytes. Abstinence from food, if not very pro-

longed, seems to increase their number in the blood (9, 10); perhaps this accounts for their greater frequency in the blood of winter than of summer frogs. They are present in the blood of the fœtus from an early period of intra-uterine life. They are especially numerous in bone marrow, and it has been urged by Ehrlich that those in the blood come from the bone marrow. Kanthack has, however, shown that they can increase enormously in a limb from which all bone marrow has been removed; and it must be remembered that large islets of them occur also in the mesentery; that they can be found forming an almost unbroken sheet in the capsules of many lymphatic glands; and that they crowd the choroid plexuses of the cerebral ventricles (18). They are numerous also in the mucosa of the small intestine, and it is said especially so after chemical irritation of the membrane. Nevertheless, in view of their resemblance in many microchemical reactions to the substance composing chromocytes, it is interesting to find them especially abundant in the very tissue which is most concerned with the production of chromocytes, *i.e.*, the red marrow.

Some investigators, among them M. Heidenhain, look upon the coarsely granular oxyphil leucocyte as a cell undergoing degeneration or over-ripe. Apparently the character of the granulation of the cell-body inclines them to this view, by the same line of argument as we may sometimes legitimately follow when judging a cell to be degenerating because it contains fatty granules in it. It is noteworthy that the opinion is shared chiefly by those whose papers deal only with the morphological characters of the cell, and with it in preparations hardened and stained. It cannot, I think, be shared by any one who has studied the cell alive and active on the warm stage or in transparent tissues. To those who are acquainted with it under approximately normal conditions such a view is negatived most emphatically by the robust reactions and the high resistance to adverse conditions, *e.g.*, irrigations with weak and strong salines, etc., exhibited by the cell. Although there is every reason for rejecting the view that the oxyphil leucocyte is a degenerated or over-

ripe cell, it is not improbable that the oxyphil granule may be derived more or less directly from worn-out nuclear substance. In the testis the cells undergoing degeneration and karyolysis in the course of spermatogenesis come to contain within the shrunken cell-substance a material highly refracting and oxyphil; this substance collects in proportion as the chromatin disappears (46). Now we know that nuclei are rich in phosphorus, and that chromatin contains iron (15); and that the oxyphil granule of the leucocyte also contains both phosphorus and iron; a chemical connection between the degeneration of nuclei and the origin of oxyphil granules is, therefore, perfectly possible. It is further worth remembering that in the red marrow, with its wealth of hæmoglobin and of oxyphil granulation, there is normally a remarkable frequency of degenerate cell-forms, and the correlation of the three occurrences is well exemplified in certain pathological conditions.

In that type of leukœmia in which the spleen and bone marrow are predominantly affected and enlarged, a great number of cells which resemble the coarsely granular oxyphil cell appear in the blood. These cells are not however exactly like the normal coarsely granular leucocyte, for they are not amœboid (20), and present other minor points of difference; they resemble more closely a free cell common in marrow, which contains oxyphil substance and is not amœboid. They certainly appear to be marrow cells which have entered the blood, but it is not proven that they are identical with the coarsely granular leucocyte of blood, or that the latter is derived from the red marrow.

V. *The Finely Granular Basophil Cell.*

This is a small cell, spherical in shape, often possessing an irregularly lobed nucleus. The cell-body contains a number of minute granules which take on an intense purple tint under the action of methylene blue. The cell is a sparse but probably a normal denizen of the blood; in man it is said to be least rare in the blood in about the third hour of digestion (2), but even then it requires long searching to discover. It has not been met with in the lymph or serous

chambers, but we have at present very few facts concerning its distribution. It is certainly sometimes present in excess in the blood of uræmic patients (Grünbaum).

VI. *The Coarsely Granular Basophil Cell.*

This is one of the largest and most striking forms of leucocyte. It is in figure usually a somewhat flattened sphere; its nucleus is round and central. The granules with which the cell-body is densely laden are large, about 1 μ diameter, and are, unlike the oxyphil granules, not highly refracting. The cells themselves are more fragile than most leucocytes; they break or burst, and let loose their granules under even careful modes of preparation. They have for this reason been sometimes termed "explosive cells". They are not amœboid, nor, as far as is known, phagocytes. They form about a tenth of the collection of free cells in lymph and serous fluids. They are completely absent from the blood. They are identical, as far as their granulation is concerned, with certain fixed cells which are numerous in the connective tissue around lymph and blood-vessels—Ehrlich's *mast zellen.* Islets of such cells can be found in the mesentery accompanying certain blood-vessels. They are numerous also in the mucosa of the small intestine. To obtain striking specimens of the coarsely granular basophil leucocyte no better *locus* can be chosen than the pericardial chamber of the rat. In the moisture of that sac extremely fine examples of the cell are easily found.

It is clear from the foregoing that the morphological differences between these various leucocytes are marked enough. Some observers hold that the various forms are not distinct species of cell at all, but are merely the various aspects assumed by one and the same pleomorphic organism in successive phases of its individual life history. Such a speculation suggests itself very naturally, but is not so easy either to prove or to disprove. The "pleomorphism" view is pushed to its extreme by those pathologists who assert that leucocytes can arise by transformation of fixed connective tissue corpuscles, and that then after wandering through a

period of amœboid and vagrant existence they can settle down once more as fixed corpuscles, and produce the fibres and the variously complicated matrix of dense connective tissue, *e.g.*, in the repair of wounds. As a possibility this view is tenable, but it has been shown not to hold good in all the instances in which the sequence of events has been examined carefully (21).

That the coarsely granular basophil leucocyte and the two forms of oxyphil leucocytes must all three be considered specifically different individuals there is little reason to doubt. Whether the two forms of hyaline leucocyte are not stages of the same cell, and whether one of them or both may not be young forms of the finely granular, are possibilities that seem not improbable. Apart from the not very numerous coarsely granular oxyphil leucocytes and the much rarer finely granular basophil, the leucocytes of the blood have been classed by Ouskow (22) and Khetagorow (23) as (1) young elements, (2) ripe elements, (3) over-ripe elements. Among the young elements they place the small hyaline cells (lymphocytes) and some hyaline forms not much larger than lymphocytes. By ripe elements they mean the large hyaline forms and certain others transitional in appearance between those and the finely granular oxyphil; these last are their over-ripe elements. They believe the lymphocytes enter the blood from the lymph glands and from the spleen, and the somewhat larger hyaline form from the red marrow. These views are at present, however, conjectural rather than based on any actual demonstration.

As to the life history of the coarsely granular oxyphil cell there is a small element met with in serous fluid and blood which is probably the young form of this cell. It has a spherical nucleus and a scanty amount of cell-substance containing a few granules of the typical appearance and reaction. Similarly it is fairly easy to trace the individual growth of the coarsely granular basophil cell from a small cell somewhat like a lymphocyte, except that its scanty cell-substance contains the characteristic basophil granulation; every intermediate form up to the large "mast

zelle" can by a little searching be discovered. The existence of these various forms of young cell argues strongly against pleomorphism explaining the existence of the different adult forms.

Two facts rise saliently out of the investigation of the granulation contained in leucocytes. In the first place one and the same individual cell never contains at one and the same time two different kinds of granules: all the granules within it are of similar quality, as least as far as they can be tested by the microscopical reactions at present available. If the cell at one time produced one kind of granule, and at another time another, this is hardly what would have been expected. In the second place the granular material present in them is of extremely wide distribution in the animal kingdom. For instance, the eosinophil granule can be studied alike in the Vermes (24), Mollusca (25) and Arthropods, as well as throughout all classes of the Vertebrata. It would seem of very ancient origin in the evolution of animal structure, and it must possess physiological significance of some fundamental kind. What that significance may be we have at present very little opportunity of gauging; it has been suggested above that it may have to do with the formation of hæmoglobin, but the eosinophil substance is present in some animals that are not possessed of any hæmoglobin. It has been mentioned above that this granule has some bactericidal power (2), but inasmuch as that power can only be exercised under abnormal conditions it is not to be considered an attribute which explains the normal production of the material.

An inquiry may be raised as to how far it is permissible to consider cells to be necessarily of the same kind or closely genetically connected simply because they contain granules of the same material. If, for instance, the occurrence of fat in cells be used for argument, in the same way it may be urged that certain liver cells and certain connective tissue corpuscles are closely allied structures. Certainly some connective tissue corpuscles contain coarse oxyphil granules of appearance and quality similar to those in the

wandering cells; it does not however appear to me on that account permissible for us to consider that the cells themselves are to be classed as similar.

Study of the varieties of leucocytes brings out clearly the fact that in the wandering cells taken as a group digestive activity is prominent, and that the secretion of digestive juices is a function highly developed in them. In some varieties the secretion is poured out into a special "vacuole" in which the food or prey is by the motive power of the cell surrounded; the digestion takes place then in what may be called an "intracellular" manner. The phenomenon termed "phagocytosis" is a particular instance of this "intracellular" digestion. Metschnikoff, whose attention has been largely devoted to this subject, considers that in "phagocytosis" we have the primary and central fact not only of many normal processes, but also of the great pathological process of inflammation; he even defines inflammation as a "phagocytic reaction," and attempts to explain all resistance to and immunity from infectious disease by means of this particular phenomenon, limited though it is to certain varieties of cell. This narrow view leaves out of consideration reactions which are, at least as constantly as "phagocytosis," a part of the inflammation process, and of immunity and resistance. Furthermore, strangely enough it overlooks altogether the more general and more potent extracellular method of digestion in which the cells attack food and prey in their immediate environment, dissolving these without incepting any particles of them. This is the process which reaches its extreme perfection in the secretory epithelia of the various special digestive glands, such as the salivary and intestinal; but it is recognisable in unicellular organisms, and, as above mentioned, in the coarsely granular oxyphil leucocyte.

I should extend the length of the present article too greatly were I to attempt here any account of the various forms and grades of "leucocytosis," that is to say, of numerical increase of the wandering cells collected in a part. Could we include such a description within this

scope, more interesting, and even more obvious than the above outline indicates, would appear the differences between the various kinds of leucocytes alike in regard to function and to structure.

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COAL: ITS STRUCTURE AND FORMATION.

PART II.

STRICTLY speaking, the subject of microscopical structures in coal should include some account of the sphærosiderite nodules which occasionally occur in seams of coal. It is from these nodules that Binney, Carruthers, Williamson and others have obtained material for the anatomical investigation of Coal-Measure plants; the majority of the invaluable treasures contained in Professor Williamson's unique collection of Carboniferous plants have been prepared from such nodules. According to Stur we should regard these small nodular masses as patches of partially decomposed plant substance, mineralised by carbonated waters, and thus preserved as sample specimens of a peat-like accumulation which was, for the most part, gradually converted into coal.[1] In the Reichsanstalt museum, in Vienna, there is an exceedingly interesting specimen of a block of coal containing elliptical or spherical nodules of sphærosiderite, with some pieces of crystalline igneous rocks embedded in a matrix of coal. Stur considers that these mineralised patches of peat lend considerable support to the growth-in-place theory of coal formation; but, as Grand' Eury has pointed out, it is not difficult to understand the mineralisation of nests of plant tissue by the action of petrifying solutions in a mass of subaqueous vegetable *débris*. The occurrence of boulders of different kinds of rock in coal beds is a subject of great interest, but it is impossible to give any adequate summary of the recorded facts in the present incomplete sketch.[2] Some geologists, with their firm belief in the autochthonous formation of coal, suggested a meteoric origin for such stones; others adopted a more rational view, and regarded them as erratic blocks which had been carried by floating trees or ice, and finally dropped into a peaty deposit on the floor of a lagoon. The occurrence of the so-called coal balls is

[1] See also Binney, p. 14. [2] For references, see Geikie and Stur.

of considerable interest; these are rounded pieces of coal occasionally met with in seams of coal of a somewhat different physical nature to the included balls. Logan long ago mentioned their occurrence in South Wales, and Renault, Fremy and others have since described them at some length, and discussed their probable manner of formation in the case of the French Coal-Measures. These balls are frequently more porous than the coal in which they occur; they are looked upon as water-worn fragments of older carbonaceous strata. Renault and others make use of the coal balls as an argument in favour of the rapid carbonisation of vegetable deposits; they believe that certain beds of plant material in the Commentry basin were converted into a more or less porous form of coal, and suffered disintegration prior to the complete deposition of other seams in the same area.

Whilst some geologists have preferred to regard coal as an advanced stage in the gradual compression and chemical alteration of peat mosses, others have held fast to the idea that each seam of coal marks the site of a thick mass of vegetable accumulation, derived from a dense and long-continued forest growth on a gradually subsiding area. "The same area was alternately covered with vast forests, such as we see in the deltas of great rivers in warm climates, which are liable to be submerged beneath fresh or salt water should the land sink vertically a few feet."[1]

Geikie, in the recent edition of his text-book, briefly describes this autochthonous method of coal formation, but goes on to devote a few lines to Fayol's work in the Commentry coalfield of France, and admits the strong case which this experienced geologist has founded on his detailed researches. Geikie concludes by expressing the opinion that "it would thus appear that no one hypothesis is universally applicable for the explanation of the origin of coal, but that growth on the spot and transport from neighbouring land have both, in different regions, contemporaneously and at

[1] Lyell (1), p. 388. (Good figures of "spore coal" sections are given on p. 405.)

successive periods come into play". In his excellent *Traité de Geologie* De Lapparent[1] enters more fully into the question of coal formation, and states, at some length, the arguments advanced by recent workers in favour of the drift theory or allochthonous origin of coal seams. The tendency in England seems to have been much too conservative with reference to this matter, and it is only by slow degrees that any adequate recognition has been accorded to the strong case against the old orthodox opinions as to the formation of coal. In Solms-Laubach's encyclopædic text-book on fossil botany we have an extremely interesting account of the present state of our knowledge as to the nature of coal and its manner of production. A sketch is given of Grand' Eury's views, and the geographical and physical conditions are depicted, which seem to have obtained according to the comparatively recent theory of this French savant. In 1882 a full account of Grand' Eury's views appeared in the *Revue de deux Mondes*, written by the ready pen of the Marquis of Saporta. Before noticing some of the more important points in Grand' Eury's arguments, reference must be made to a much earlier work by an English geologist. The importance of Beete Jukes' studies in the South Staffordshire coalfield does not appear to have been adequately recognised by English geologists, but in the works of their French *confrères* we find frequent references to this valuable survey memoir. One of the most striking features exhibited by the Staffordshire coal beds is the splitting of a thirty-foot seam, when traced in a northerly direction, into ten to fourteen distinct seams, separated by intervening beds of sandstones and shales. A section of the strata in one area shows a mass of coal thirty feet in thickness, in another section, not more than five miles in a horizontal direction from the first, the thirty-foot seam has become sub-divided into several beds, which are interstratified in a series of sedimentary rocks three hundred feet thick. This splitting of beds towards a particular direction is "a kind of change quite familiar to those who are accustomed to trace any set

[1] P. 860.

of beds continuously along the strike,"[1] and the explanation of the same kind of phenomenon in a series of ordinary sediments would involve no exceptional conditions of sedimentation. A river flowing into an open lake or sea would deposit a mass of sand and mud near its mouth, but in the deeper and clearer waters the deposits would be made up almost entirely of fine sediment, which would be spread out over a much larger area; thus building up in the whole area of sedimentation a series of beds, of which the heavier and coarser materials would gradually preponderate in a direction towards the source of supply. Regarded, therefore, as an ordinary series of sedimentary deposits, the Staffordshire sections are not difficult to interpret, but if we must adhere to the theory of an intermittent subsidence taking place over one part of a small area, and not over another, the reading of the sections becomes a matter of much greater difficulty.

This splitting of coal seams, which occurs on an unusually large scale in the Staffordshire coalfield, is of frequent occurrence in other districts.[2]

The theory of a gradually sinking area, with periods of subsidence alternating with intervals of rest and luxuriant forest growth, was formulated by Bowman in 1840, and has been accepted by Lyell and the majority of geologists as a satisfactory method of explaining the blending and splitting of coal seams. The more reasonable point of view is surely that adopted by Beete Jukes, Grand' Eury, and other writers, which regards the seams of coal as beds of vegetable sediment, deposited under the same conditions as strata of arenaceous and argillaceous materials. The regularity and uniform character of coal beds is occasionally interrupted by the occurrence of sandstone patches and other foreign material, constituting the "rock and rig" or "rolls and swells" of English miners. Such patches of sandstone offer no great difficulty to the geologist who looks to the ordinary rules of sedimentation for an explanation of the facts; the "rolls and swells" are simply ridge-like accumulations of sand or mud piled up on the floor of the area of deposition.

[1] Jukes, p. 19. [2] See Grand' Eury (2), Pls. xxii. and xxiv., etc.

Grand' Eury[1] describes several instances of the same kind, and, like Beete Jukes, draws attention to the fact that the coal was laid down in horizontal beds, none being deposited on the crests of the sandstone ridges until the whole floor had been raised to that level. This method of deposition points to the existence of currents rather than a quiet sheet of water in which the sediment would slowly sink to the bottom, and fill up all irregularities of surface contour. The existence of the numerous thin laminæ in coal is another fact which influenced Jukes in his choice of the drift theory of coal formation. Attention has recently been directed to this laminar structure by another advocate of the allochthonous origin of coal seams.[2]

Grand' Eury's exhaustive memoirs on the foundation of coal supply us with a considerable amount of evidence collected by one intimately acquainted with the geology and botany of the upper carboniferous rocks; and the facts which he brings forward clearly point to the subaqueous deposition of layers of carbonaceous sediment. He does not favour the view that the vegetable *débris* was drifted for a long distance from forest-covered land, but considers the facts more readily explained on the assumption of a comparatively short transport. Grand' Eury's views are thus concisely stated by Solms-Laubach:[3] "Coal seams were formed in broad land-locked lake basins (lagoons) surrounded by wooded swamps, in which the decaying vegetation, softening and rotting as it lay on the ground, produced in time a layer of matter of vast thickness. The water of frequent rain-storms running slowly off in trickling streams gradually carried away with it the softened wood in shreds from inside the encasing rind, which was itself ultimately broken up and conveyed with other deposits into the basin. Here the processes which lead to the formation of coal took the place of decay, the mass of the coal being produced from the rind, while the particles of softened wood were converted into fibrous coal. The masses of aquatic and marsh plants, which covered the surface and margins of the basin with

[1] Grand' Eury (2). [2] Gresley. See also Goodchild. [3] P. 23 (1).

their luxuriant growth, also supplied their contingent in the form of the parts which died and sank to the bottom."

Without attempting to give any full account of the facts on which this particular theory has been founded, a brief summary of the main arguments, advanced by the supporters of the allochthonous origin of coal, may serve to draw attention to some of the more recent views which, on the whole, seem more adequate to explain the method of coal formation than those usually accepted. If the plant fragments, from which coal has been formed, had gradually accumulated on the floor of coal period forests, we should expect to find in them more commonly a close association of fragments of the same species or genus of plant, but, as a rule, we have a heterogeneous assemblage of all kinds of plants, much more suggestive of drifted vegetable *débris* than a collection *in situ* of forest vegetation.

In a mass of plant *débris*, covering the site of a former forest, we should look for the occurrence of large stems, and more complete specimens of leaves or branches, instead of a fine vegetable paste containing a few scattered fragments of plant tissues.

The nature and manner of occurrence of the underclay stigmarias are matters of great importance from our present point of view. The serious difficulty to be explained by the supporters of the growth-in-place theory is the absence in nearly every case of the stems of *Sigillaria* or *Lepidodendron*, in direct connection with the dichotomously branched stigmarias. One or two instances have been recorded of sigillarian stems passing up into the coal from a stigmarian root in the underlying rock, but there is a striking absence of satisfactory instances of such a mode of occurrence of these plants.

Grand' Eury regards many of the stigmarian fossils as aquatic rhizomes, and in several instances he admits there can be little doubt but that they occur in place. Potonié[1] has recently called attention to the manner of occurrence of *Stigmaria*, with its delicate appendages radiating in all directions

[1] Potonié (1).

through the unstratified underclay, as a strong argument in support of the autochthonous views; but he apparently overlooks the fact that it is generally admitted that these plants are often found in their position of growth, a mode of occurrence which does not at all preclude the deposition of coaly sediment on a submerged floor, penetrated by roots or creeping rhizomes. An old land surface may easily become covered with water, or form the floor of a lake in which vegetable sediment is being accumulated, or, as some hold, the stigmarian soils may very likely have been under water during the growth of the plants. It must not be forgotten that in some districts stigmarian underclays are not found below seams of coal, and even in England their occurrence is by no means universal. On the other hand it not unfrequently happens that the underclays are overlain by grits or sandstones, and not by a bed of coal. The frequent irregularity in the floor surface of a coal seam, and the distinct unconformity between the coal and the underlying rock, clearly points to an interval between the deposition of the floor rock and the formation of the coal. Denudation must have taken place in this interval between the deposition of the two sets of beds, and the stem portions of the trees removed before the carbonaceous sediments were laid down on the submerged underclays. Grand' Eury includes in the family of *Stigmareæ* two forms of plants: the true stigmarias he regards as rhizomes, which floated in water or grew on the surface mud, into which their branches penetrated; the true roots of *Sigillaria* he speaks of under the name *Stigmariopsis*, and sees in them certain well-marked structural peculiarities. Some further light has lately been thrown on this vexed question by the results set forward in an interesting memoir by Solms-Laubach;[1] but a detailed consideration of the stigmarian question must be deferred to a later article.

The occurrence over large areas of uniformly pure beds of coal is frequently urged against the theory of drifted vegetable *débris*. This fairly constant character in a wide-

[1] Solms-Laubach (2).

spread deposit is by no means confined to coal seams; among the Palæozoic strata sheets of fine-grained muds may be followed over considerable tracts of country without any great change in lithological structure. If we take into account the lightness of vegetable fragments, and of the ulmic substances which would be carried away by water flowing over the partially decayed accumulations on forest-clad surfaces, it is not difficult to understand how such sediments might have been spread out over an area of wide extent, with little or no admixture of heavier detrital material. Fayol's instructive experiments[1] on the conditions of sedimentation fully bear out the assertion that in many coal basins the manner of occurrence of coals, sandstones, and shales, is exactly analogous to that of a series of subaqueous deposits laid down in a quiet fresh-water lagoon, or on the floor of a current-swept area of deposition. The same observer has made out a very good case with regard to the vertical stems well known to those familiar with Coal-Measure stratigraphy; many of these have undoubtedly been drifted, and have finally settled down in the sand or mud in an erect position. Instances of calamitean and other trunks preserved where they grew in a swampy or submerged region are by no means unknown. Grand' Eury and other French writers have figured several examples of such stems, showing a series of adventitious roots developed at successively higher levels on the axis, and radiating out into the surrounding and growing sediment.

The enormous amount of time necessitated by the growth-in-place theory would seem to be a matter of some difficulty. Geologists have been warned by physicists, whether on good grounds or not cannot here be discussed, that it is no longer possible to draw unlimited "time cheques" in support of any theory which involves long periods of time. The sedimentation method of formation does not make any such exorbitant demands on the duration of the coal-producing era. Many authorities have laid stress on the gradual passage of pure coal through impure

[1] P. 365, *et seq.*

varieties and carbonaceous shale to ordinary shale or grit; and, indeed, the stratigraphical evidence unmistakably suggests that we must look upon beds of coal as units in a series of strata built up on an area of deposition according to a common law of sedimentation. The chemistry of coal formation is too wide a theme to deal with in an article which has already extended beyond reasonable limits. The processes of change seem to have gone on much more rapidly than is usually supposed, if we endorse the conclusions of Renault and others. Some writers consider the coaly transformation of plant tissues must have required not only pressure, but a considerable amount of heat. Others, again, believe that a mass of decaying vegetable matter has undergone a kind of peaty fermentation, resulting in the partial carbonisation of the tissues, and the production of various ulmic substances. This vegetable sediment would be spread out on the floor of a lagoon, and eventually covered with mud or sand, when the further stages in the process of carbonisation would be completed by a gradual desiccation of the whole mass under some pressure and a comparatively low temperature.

Much has been written as to the reduction in bulk of vegetable tissues as a result of their conversion into coal. The late Dr. Stur attempted to estimate the amount of decrease in volume, and suggested a simple formula by which to calculate the original diameter of a plant's stem or root, from the thickness of the coaly layer which frequently envelops a pith cast in sandstone or shale. Calculations have been made by other palæobotanists, which differ considerably from those of Stur, but as Potonié[1] safely suggests, we cannot expect to arrive at any numerical estimate of the volume reduction which may be applied to all cases.

Neglecting for the present the probable climatic conditions which characterised the coal period forests, and the discussion of the biology of coal-forming plants, we may, in conclusion, make brief reference to a few of the numerous descriptions of recent geographical conditions which have

[1] Potonié (2).

been quoted as possible parallels to those of the coal age. M. Rigaud looks to the pitch lake of Trinidad and similar places as the nearest representatives to-day of Coal-Measure conditions; Lyell and others have referred to the great Dismal Swamp of Virginia as the most accurate picture of coal forests and the accumulation of plant *débris*. An interesting account of this famous North American region has lately been published in one of the annual reports of the United States Geological Survey.[1] The upholders of the drift theory, as propounded by Fayol, point to the delta deposits of the Mississippi and other rivers, which transport enormous masses of vegetable material; and a more recent writer, Ochsenius, calls special attention to the Pemisco lake, and other similar sheets of water in close connection with the Mississippi channel, as very likely places for coal building. Lyell's description of the Mississippi[2] and its drifted rafts of timber enables us to realise at least one possible manner of vegetable accumulation in a delta deposit, or in lagoons bordering the main stream. The well-wooded swamps on either side of the river banks serve to filter off the coarser materials from the overflowing waters in the flood seasons, thus allowing the finer muddy sediment to be carried for a greater distance, and spread out as "a stiff unctuous black soil which gradually envelops the basis of trees growing on the borders of the swamp". Grand' Eury's view of a short-distance transport necessitates a state of things very difficult to parallel at the present day. Stur, Lesquereux, and others, regard the coal beds as Palæozoic peat formations, and, arguing from this analogy, Neumayr maintains the probability of a temperate rather than a tropical climate during the Permo-Carboniferous epoch. It is easy to recognise in many river deltas conditions similar to those which doubtless obtained during the deposition of beds of cannel coal and other carbonaceous sediments, which bear clear indications of subaqueous formation. Some of the widespread sheets of fairly pure coal may probably be best explained by some such concurrence of conditions as Grand' Eury has

[1] Shaler. [2] Lyell (2), vol. i., chap. xix.

suggested, or in other cases we may find a satisfactory analogy in the extensive sheets of vegetable mud spread out on the floor of a lagoon or lake after transport by the raft-covered waters of a river.

The peat bog method of formation, or the accumulation and subsequent sealing up of vast thickness of forest *débris*, present serious difficulties to be overcome, and require certain conditions which we are not warranted in assuming for upper carboniferous times. A certain proportion of the material, which has played a part in coal formation, was, in all probability, derived from floating vegetation, and from the *débris* of plants growing on submerged ground. The description by African travellers of the dense masses of floating weeds and soft pulpy vegetable material which prove an efficient barrier to navigation, affords a picture of one possible source of vegetable sediment which might, under certain conditions, be converted into coal.

Mr. Graham Kerr has been good enough to draw up the following brief account of the floating vegetation in some South American waters; such conditions as he describes may well have occurred during the coal-producing age. "A very large area of the more central parts of the 'Gran Chaco' is covered by wide-spreading swamps and extensive lagunas. The latter are normally of great extent, and are in many cases completely covered with a floating carpet consisting mostly of *Azolla* and *Pistia*, often accompanied by *Pontederia*. As the periodic dry seasons come round, the lagunas shrink in volume, and the floating carpet is gradually let down until it finally rests on the ground. The *Azolla* develops its sporangia in great numbers, but the plants themselves mostly rot away, and in the black deposit of the laguna floor one finds their remains, including the numerous sporangia. The rivers of the Paraguay-Paraná system are subject to periodic 'crecientes,' in which the level of their waters often rises some thirty feet. During these periods of high water one meets with enormous floating islands made up of tree trunks, masses of *Pontederia* and other plants, the latter of which have been floated out of the lagunas of the Chaco, etc. These floating islands carry down with

them numerous tropical plants and animals. Cases have been recorded in which even jaguars and other large mammals have been carried down to the lower reaches of the Paraná and the region of its delta."

Other conditions, which recall Fayol's description of the Commentry Permo-Carboniferous lake basins and the water-borne vegetable sediment, have been described to me by Mr. Philip Lake, late of the Indian Geological Survey. On the floor of the Travancore back waters of Southern India, a very carbonaceous deposit is being formed from the material carried to the sea by rivers which have flowed through a forest-covered country ; these extensive beds of fairly pure vegetable sediment, in process of accumulation, may well be compared with the first stages in the building up of coal seams. Dr. Gregory, in a paper on the Norfolk Broads, would have us look nearer home for a modern illustration of coal-forming conditions. He suggests that a study of the Broads "enables us to follow in detail the history of a great estuary, and it presents us with, perhaps, the closest analogy to the conditions of the formation of our coalfields".

We may sum up the whole matter by expressing the conviction that the weight of evidence seems to tip the balance of opinion very materially towards the theory of drifting, and subaqueous sedimentation, for the majority of the Palæozoic coal seams. Some coals are probably old peat bogs or similar autochthonous formations, which have passed into the state of coal as the result of favourable physical conditions. But while suggesting the allochthonous manner of formation as the most widely applicable, we may conclude with the saving clause that "die Natur nicht alles über einen Leist geschlagen hat".[1]

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ON DIGESTION IN THE CŒLENTERA.

THE science of Comparative Physiology is one which has not yet received the attention which it deserves from those engaged in biological investigations. The facts of the anatomy and development of a large number of animals are now well known, but we are still in possession of very little information concerning the physiological processes of digestion, respiration, circulation and the like of the commonest and best known creatures which are dissected in our laboratories.

A special word of welcome, therefore, must be given to any piece of careful and conscientious work in this line of research, and the hope expressed that the work will be continued and extended.

The subject to which some attention has been paid during the last few years, and in which it may be said some little progress has been made, is that of the digestive processes in the Invertebrata.

Although the investigation received its most important stimulus from the researches of Metschnikoff on the digestion of various groups of invertebrata, the interest has been somewhat concentrated of recent years upon the endeavour to determine accurately the physiology of the digestion of Cœlentera, and it may be of some interest to the readers of this journal to have in the form of a short essay some account of the more recent investigations on this branch of the subject.

In the year 1880, Metschnikoff proved that the endoderm cells of many hydroid polyps, sea-anemones and other Cœlenterates possess the power of throwing out processes of their protoplasm, embracing particles of carmine and the like, and thus incorporating them into their substance. These particles of carmine are not of course digested by the endoderm, but the conclusion was drawn that the cells have in like manner the power of enclosing

small particles of food and digesting them within the cell-substance, just as an amœba is known to catch and digest its food particles.

The process of digestion within the cell-substance, or "intracellular digestion," was subsequently discovered by Metschnikoff and other observers to occur in the intestine, or in certain regions of the intestines, of other Invertebrates, and may be said to be now recognised as a normal method of digestion in Cœlentera, in many worms, and probably also in other groups of animals.

This fact being established, the next questions which arose were: Is this the only method of digestion in Cœlentera? Does no digestion take place in the cavity of the polyps? Is there no extracellular digestion? The first question had to be answered in the negative, and the last two in the affirmative. No one who has seen a Hydra catch and devour a large daphnia can doubt for a moment that some fluid is brought to bear upon its tissues which leads to their rapid disintegration, and to assert that this process occurs within a single endoderm cell, or plasmodium of fused endoderm cells, would be erroneous and ridiculous.

The next stage in the history of the investigation was reached when Lankester, in 1881, published an account of his researches on the digestion of Limnocodium and the structure of its endoderm.

Limnocodium is the name given to the interesting little freshwater medusa which periodically makes its appearance in the Victoria Regia tank in the Botanical Gardens in Regent's Park. The mouth of this delicate little creature is situated at the end of a quadrangular tube suspended from the centre of the under side of the disc. He found that the endoderm lining the walls of the cavity of this tube or stomach presents three principal forms, which occur in the oral, the mid-gastric and the proximal regions respectively.

Intracellular digestion was observed in the proximal region only; small unicellular organisms being enclosed in the substance of the cells, some apparently unaffected and others in various stages of disintegration and dissolution.

In the oral region of the stomach the endoderm was of a different character and never contained food particles. In those forms which were, in all probability, in a hungry condition, numerous large clear goblet cells were observed, which were shed apparently in the specimens captured in the act of digestion.

The conclusion was therefore drawn that, in addition to the ordinary process of intracellular digestion, a secretion may be poured into the cavity of the stomach from the goblet cells, or secretion cells as they were more correctly termed, which probably exerts some digestive action upon the larger forms of prey, *outside* the cell-layers.

In other words, in addition to the process of intracellular digestion, there is a process of extracellular digestion, the latter being perhaps only preparatory to the former.

A few years later Jickeli and Nussbaum described in the endoderm of Hydra, in addition to the ordinary large vacuolated cells, certain smaller forms which were called gland cells.

A physiological investigation of these cells led Miss Greenwood, in 1888, to the conclusion that the digestion of the food of Hydra takes place entirely outside the endoderm cells, that the small pyriform bodies found in gland cells are poured into the body cavity during digestive activity in the form of a fluid secretion, and that the products of digestion are taken up by the large vacuolated cells in a fluid form for storage and subsequent distribution.

These results, then, though differing in some minor points from those obtained by Lankester in Limnocodium, confirmed his conclusion as to the secretion of a fluid into the cavity of the Hydroid by certain cells of the endoderm.

Further evidence upon this point was obtained by Hardy, who made an elaborate investigation of the endoderm of Myriothela phrygia. He found that the food of this Hydroid, which consists chiefly of small Crustacea, is digested at first in the lower portion of the tentacle bearing region where the gland cells are most abundant, and is due to a digestive fluid poured into the cavity and secreted by

those gland cells. The result of this digestion is the disintegration of the food into a number of fragments which float freely in the fluid of the cavity, and the solution of a considerable quantity of the proteids of the prey which may be precipitated by treatment with corrosive sublimate. It was further shown that many of the fragments are taken up by the endoderm cells and digested within the cell substance, whilst the vacuolated cells of the endoderm become loaded with granules—the so-called nutritive spheres—which are formed from the products of digestion in solution in the cœlenteric cavity.

The general results of these investigations have proved then that in the Hydroids two kinds of endoderm cells occur, namely, secreting gland cells and vacuolated cells for the absorption and storage of the product of digestion in the fluid form, and that in some Hydroids, but not apparently in Hydra, the intracellular digestion of a portion of the food may occur.

In the Hydrozoa the cavity of the body is simple and continuous, and although different regions may be defined in which the two or more varieties of endoderm cells may be predominant, no special digestive organs of definite shape can be recognised upon simple dissection and examination with a lens.

In the Anthozoa, however, a more complicated structure is observed. The mouth opens into a short tube, which is ectodermic in origin, called the stomodæum, and this is connected to the body wall by a number of partitions or mesenteries lined on both their surfaces by endoderm cells. Their free edges are somewhat convoluted, are provided with a thickened epithelium, and are termed the mesenterial filaments.

The question then naturally occurs: "Have we in this more complicated structure some separation of the cells which in the Hydrozoa perform the functions of digestion?" Or to put the same question in other words: "Does the epithelium of the mesenterial filaments perform the function of secretion or absorption only, and the remainder of the endoderm lining the body walls and mesenteries the

supplementary one, or are the functions referred to not confined to any one region or set of organs in the Anthozoan anatomy?"

Concerning the function of the stomodæum we have very little experimental evidence.

The Hertwigs and others have shown that the epithelium lining the inner walls does contain among the columnar ciliated cells of which it is mainly composed a certain number of gland cells which are histologically of two kinds, but to what extent these cells assist in the digestion of the food has not been experimentally determined.

In the Alcyonaria no gland cells are found in this region, and it seems probable that the stomodæum in all Anthozoa is simply a food passage, and plays, at most, a very small part in the process of digestion. In all Anthozoa, however, the epithelium of the mesenterial filaments—with the exception of the two dorsal ones in Alcyonaria—is crowded with gland cells, and there can be no doubt that they secrete a fluid which disintegrates and dissolves the food.

When the food has passed through the stomodæum, it may be seen in certain transparent polyps to be seized by the filaments and held in their grasp until it is disintegrated and partly dissolved. Krukenberg described the filaments of sea-anemones as being wound about the food, and Wilson found that in Leptogorgia virgulata—an Alcyonarian—the food was "held closely clasped by the mesenterial filaments for two or three hours," and that afterwards "a mass of refuse matter was passed out through the œsophagus," *i.e.*, stomodæum, "and the filaments resumed their normal position".

There can be little doubt, then, that these organs do secrete a digestive fluid in the Alcyonaria, and there can be little doubt that the gland cells which secrete this fluid are chiefly confined to these filaments. But is the absorption of the food either in a fluid state or in a particulate form confined to the cells of the filaments, or to the cells of the general endoderm lining of the body cavity, or does it occur

in both the filaments and the general endoderm? Upon these points there is conflicting evidence. Wilson and others found certain diatoms and other foreign bodies embedded in the epithelium of the filaments, and failed to find them in the endoderm cells of the mesenteries and body wall, and consequently the former came to the conclusion that probably "the digestive functions are performed by the entodermic filaments alone, and never by the ectodermic filaments or the general entoderm".

A little consideration, however, would have led the American observer, one cannot help thinking, to a different conclusion; for he himself and the writer of the present article, working quite independently of one another, discovered that a regular circulation of the fluids of the cœlenteron in Alcyonaria is effected by the long cilia of the groove on the ventral side of the stomodæum producing a current from without inwards on the one side, and by the cilia of the dorsal or ectodermic filaments producing a current in a reverse direction on the opposite side of the cœlenteron.

Now such a current, constantly at work, must immediately drive the fluid products of digestion away from the region of the filaments, and they would consequently be entirely lost to the animal unless they were absorbed by the general endoderm. Moreover, it is difficult to understand how the lower parts of a massive colony such as Alcyonium could receive any nourishment at all if the absorption of the food occurs only in the filaments, unless indeed we suppose that nourishment may be handed on from cell to cell for two or three feet of endoderm.

Some recent investigations of Willem on the digestion of sea-anemones leads us to the conclusion that the absorption of the food is chiefly, if not solely, confined to the general endoderm, the epithelium of the mesenterial filaments taking but a small share in it. By feeding certain anemones with carminated albumin, or with finely chopped liver of mussels, the yellowish-brown fatty globules of which can be easily recognised in the substance of the cells, and then examining the endoderm by a lens or by

means of sections, the German observer was able to prove that both on the mesenteries and the general body wall the endoderm cells do swallow, probably in an amœboid manner, the food that is given to them in a finely divided state.

The general results obtain by Willem have been confirmed by Chapeaux, and our knowledge of the process of digestion extended by an investigation of the chemical character of the digestive fluid secreted by the endoderm, and its effect upon the food material.

Chapeaux found that when the anemone Adamsia is removed from the shell on which it lives, it emits a liquid which contains a number of refracting vesicles and fine granules. This liquid is distinctly more alkaline than sea-water, and is capable of digesting fibrin and emulsifying fats but exercises no action upon starch and cellulose. The extracellular digestion then is due to a ferment which acts in an alkaline medium.

When particles of litmus are injected into an anemone they are greedily taken up by the endoderm cells and acted upon by some acid secretion within the cell-substance. When olive oil is injected it becomes rapidly emulsified, and in the course of from five to twelve hours the endoderm cells become gorged with fatty granulations. Small particles of fibrin are in like manner taken up by these cells and rapidly digested. The intracellular ferment, however, does not, according to Chapeaux's investigations, effect any change in cellulose, and fragments of Cladophora and Ulva are rejected intact after remaining twelve hours in the cœlenteron.

Chapeaux is of opinion that the mesenterial filaments as well as the general endoderm take part in this process of intracellular digestion, but his statements on this point are not accompanied by any figures, and must be received with caution.

The results of Chapeaux's investigations, that there is an extracellular alkaline digestion of fibrin on the one hand, and a subsequent intracellular acid digestion of fibrin on the other, are undoubtedly of great interest, and will, it is to

be hoped, lead to renewed interest in this subject; but in many respects his paper is unsatisfactory. His statement, for instance, that the digestion of the gastrozooids of the Siphonophore Praya is solely intracellular is inconsistent with our knowledge of the processes of the digestion and minute anatomy of the endoderm of Hydroïd polyps, and it is surprising that it should be made without some reference to the work that has been done by other naturalists in this group. The paper would have been of greater value too if it had been accompanied by some illustrations of the process of intracellular digestion in the general endoderm and mesenterial filaments of the sea-anemones.

A great deal still remains to be done before it can be said that we have a really clear and satisfactory account of the digestion of the Cœlenterates.

It seems to be established now that an alkaline fluid is secreted into the digestive cavity of Cœlenterates which is capable of emulsifying fats and converting proteids into peptones, that a process of intracellular digestion also occurs which is accompanied by an acid secretion, and that in that process proteids are converted into peptones, fats saponified, and starch granules dissolved. But there is still some doubt as to the form in which the soluble products of digestion are taken up by the endoderm, are stored for further use, and distributed through the organism.

Valuable results might probably be obtained by an investigation of the histology of the endoderm and physiology of digestion in one of the large medusæ which occur upon our coasts, in which the gastral pouches give rise to fine canals distributed over the umbrella. It would be interesting to learn in what respects the endoderm in the marginal ring canal differs from that of the pouches, and in what form it receives its nourishment. These and other points, it is hoped, will soon be taken up, and our knowledge widened of this interesting and important branch of comparative physiology.

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FOLDS AND FAULTING: A REVIEW.

PART II.

IN the year 1884 Bertrand proved that in the region of Provence similar overfoldings or overthrusts had been produced. Renevier worked out the region of the Savoy Alps, and the country above Bex, the enormous overthrow on the Dent de Morcles becoming a standard feature in geological literature, while Schardt, by his work on the Pays d'Enhaut (10), carried us still further into the field of geological speculation. In this paper several suggestions are given, which it may be well to bear in mind. It had been asserted, more especially by Lory, that there were evidences that part of the Alpine movements, the final results of which we have already discussed, had commenced prior to the main elevation, and it is interesting to find here a case introduced, where an Eocene conglomerate, the Hornfluhgestein, contained fragments of the malm rock, of which the chains of the Rubli and Gummfluh are composed. Hence Schardt concludes that these mountains were at that time in process of formation, and formed cliffs in the Eocene sea.

Further, in the Flysch formation of this region occur a large number of blocks of exotic origin, consisting of mica-schists, talc-schists, chloritic protogine, and calcareous fragments, as also in adjacent parts, biotite-granites, quartzites, and gneisses. This indicates that in pre-Miocene times an extensive crystalline region existed, either as a continent or an island, which may even have served as the fountain-head of a not unimportant glacier, the rocks being in most cases similar to those at the present time brought down the Rhone Valley by the glacier, or distributed by the river.

It is also pointed out that whereas in the same chain any group of strata from the Lias upward has the same facies, in the chains next succeeding it has generally a different character, thus proving that submarine inequalities

must already have existed at an early period, even at the end of Liassic times.

The importance of these questions, in so far as they bear upon the existence of a distinct crystalline massif in Eocene times, will be obvious.

The main conclusion he deduces is, that from the Jurassic times onwards the submarine inequalities were already distributed in directions parallel to those of the existing chains, and that the direction of the folds had already been determined at the close of the Liassic period.

Whilst in Heim's work attention is more especially called to mountain folding as a whole, in this work now under consideration the effects of pressure on strata of different consistency are more immediately considered.

In the Pays d'Enhaut the conditions are particularly favourable for this purpose, a great mass of Jurassic and Cretaceous limestones being enclosed between Liassic marls and Eocene shales, and an examination of the pre-Alpine range reveals the fact that not merely has there been a folding of the whole, but also a differentiation of movement as regards rocks of varying composition. The Eocene beds appear to have been compressed against the Jurassic limestone strata, and as a result the former are not only vertical, but even bent back at the point of junction, the older and harder beds being thrust over the younger series.

M. Lory having in his work (11) expressed his opinion that faults preceded folds, M. Schardt re-examined the evidence, but arrived at conclusions of a diametrically opposite character. He found faults to be in the closest union with foldings, and that not infrequently faults and arches alternately replace each other; that simple vertical faults are purely superficial, never attaining any great depth, and that now dislocated overfolds are rare, owing to the great movement which the pre-Alps have undergone. Faulting is mainly represented by true, or, it may be, *anticlinal overthrust*, the one limb of an arch being thrust over the other, the throw in some cases exceeding a thousand metres. He says: "It seems certain that the overthrust of the Gastlosen chain, extending as far as the Laitmaire, is formed from a pre-

existing arch, which has not succeeded in developing itself, because it was overlaid by an enormous mass of Flysch formation, and it has therefore been disjointed, and transformed into an anticlinal overthrust".

Lateral overthrusts are more limited, and are due to the sinking of one side of an arch, the downward movement being compensated by the contortion of the marly beds forming the core of the overlying fold. In *Isoclinal overthrusts* one of the parts is so covered by the other that the beds in both appear to be nearly parallel, and where harder beds overlie softer ones, experiment has shown the possibility of a synclinal break; one of the composing anticlinal limbs being so broken as to produce an underthrust into the softer beds below (see plate ix. of above work). The final result of these studies has therefore again been to confirm, in his opinion, in a great measure Heim's views that overthrust faulting as the result of folding is a predominant rule in mountain structure.

On the basis, therefore, of a number of ascertained facts, in 1886 a very considerable amount of materials had been collected, and certain broad generalisations announced. It had been shown that mountain structure is not merely local, but the result of a movement affecting a broad belt around the globe, a considerable surface being involved, which has a distinct flow structure in a certain direction, giving rise to faults and sinking areas in the rear. That the whole of this broadly separated region has been folded and contorted, every part of the area being more or less affected by the movement, though the central portions have undergone the greatest flexuring. That normal faulting, as such, has played but little part in mountain structure; the fan structures, and overthrust folding or faulting being in the main resultants of lateral compression. That the formations of the valleys and outlines of the peaks have been entirely the work of erosion and denudation acting through prolonged periods, whilst the variations in the character of strata have given rise to all the complexities of detail which for so long a time rendered the Alpine regions a geological puzzle. It has also been ad-

vanced that in numerous cases the production of mountain masses has been preceded by the deposition of great thicknesses of marine strata, thus giving weight to the theory that a close connection exists between mountain elevation and a geo-synclinal. Yet scarcely has a rule been established, or an absolute tectonic creed promulgated, than fact upon fact pours in, either to shake our faith, or test our infallibility. Nothing seemed more certain than that the Plutonist School had suffered complete defeat and annihilation, yet in 1877 Gilbert (12) proved that there are great intruded laccolites, extending many miles in a horizontal direction, and that the stratified deposits overlying them have not only been carried upwards by this vertical movement, but have been raised a height of 5000 feet without a trace of fracture, so that Von Buch's generalisation failing to account for the greater, having been proved essentially applicable to the lesser, and vertical pressure once again claims its place as one of the formative agents in mountain formation.

Should we, again, be prepared to submit that, with the exception of mountains formed of volcanic materials erupted along a line of fissures, tangential compression has alone been active, and that mountain ranges must of necessity exhibit the complicated foldings referred to, we must again halt, seeing that a region is now known in which folding, as such, is entirely absent, Dutton and Powell having shown that the High Plateau of Utah, and the Uinta Mountains have been formed by faults, whose vertical throw has, in some cases, exceeded 7000 feet, and instead of the strata being waved, steep "monoclinal flexures" are produced.

Do we hold that folding is in the main essential where lateral pressure is active, we are face to face with Lory's assumption that faulting must be the precedent condition ere folding can be set up in mountain ranges.

What we must now ask ourselves is—whether, in the face of all these apparent counter-movements, the broad principles of folding have been affected, and lost their power or influence? Whether their fruitfulness in suggestion is exhausted, or whether their theoretical basis is strong

enough to bear the weight of a broader, and perhaps grander, super-structure? Hitherto a large gap had existed in the material upon which a wider conception could be based. The oceans themselves hid their secrets from the gaze of the anxious inquirer, and the greater portion of the solid earth crust lay unknown and uninvestigated beneath the wave. The numerous soundings made in cable expeditions, the various national deep sea enterprises, and, above all, the investigations of the *Challenger* Expedition, have at length, to a great extent, lifted the veil, and every branch of science has received an impetus which will not be exhausted for many years.

The occurrence of inequalities in the sub-oceanic area were found, not to be capricious and local, but as extensive as the great belts of elevation which gird the continents. Volcanic activity was seen to be in close relationship with the great earth features, and a wealth of material has been added to our knowledge of the fauna, temperature and character of the deposits in the great marine areas.

The conception and combination of the highest development of fold geology, and the new truths regarding our earth's geography, have been left to one of our own countrymen, Professor Lapworth, who, in his addresses to the British Association, the Royal Geographical Society, and the Geological Association, has urged his views with all the energy and fire of an unbounded enthusiasm. No longer are we dealing with mere local highlands, no longer even with belts of mountain elevation, but with one great earth fold, of which the continents, including the Atlantic basin, represent the arch, and the deep depression of the Pacific, the trough; whilst between them lies the septum, a belt of unrest and continual disturbance, that "terrestrial ring of fire," as Professor Lapworth describes it, which, from Sumatra to Chili, borders the greatest of the earth's oceans.

Geologically, there can be no grander conception than this, and geographically its unity and simplicity arrest the attention, and impress the mind. This great fold reveals itself as composed of minor folds, to which the same rule

applies ; arch and trough following each other in regular sequence, such as the arches of the Rockies and the Appalachians, with the simple trough of the Mississippi between them, or the two depressions of the Atlantic, with its central ridge, on whose steep gradients are situated the volcanic islands of St. Helena and Ascension, the Madeiras, and the Azores. And so on through the two-ridged continent of Africa, with its central table-land between ; the Indian Ocean, with its double trough ; and, finally, the Australian continent, flanked by its two ranges, the eastern and the western.

But the complexity proves even greater, the earth being regarded as triple folded in two directions ; thereby explaining the more prominent of its geographical features. Geography has given convincing answers to questions first raised on geological bases, and we are led step by step from the world-wide conception of the fold through the minuter, but no less striking, foldings, as represented in highlands and mountains, down to possible changes of ultra-microscopical dimension.

But the extension of this theory, explaining, as it does, to the satisfaction of its author every great depression and elevation on the earth's surface, demands a far wider application in dealing with the past history of our globe. There again Professor Lapworth has not hesitated to apply his method, and he postulates that, in opposition to the views held by many distinguished observers, affirming the absolute permanence of ocean basins and continental structures, there is strong evidence that our islands, for instance, have undergone successive stages of elevation and depression, the alternation between the two extremes being marked by periods of great volcanic activity, as, for example, in Ordovician and Miocene times.

A question of great importance now presents itself, *viz.*, whether these foldings are the result of mere accidental disturbance, possessing no fundamental sequence ? and Professor Bertrand (13), in seeking an answer, has inaugurated an interesting discussion.

The three main problems which he has endeavoured to solve in his work on the Paris Basin are :—

(1) Whether the Tertiary folds follow the direction of the primary ones, to which they are superposed?

(2) Have these folds been formed progressively in consequence of continuous, or slightly superadded, movements? and—

(3) Whether the system of main foldings is accompanied by one of perpendicular crumpling?

The method pursued by him is full of interest, and capable of wider application. Starting on the assumption that the Cretaceous beds have been deposited on an approximately level surface, a plane of marine denudation, he concludes that wherever these overlie the *junction* of two Jurassic beds that spot must have existed on the junction line when the Cretaceous beds were laid down. Obtaining, therefore, a series of such points over a given area, he is at length able to trace out a map of that district as it must have appeared in pre-Cretaceous times, and the study of such a map reveals the character of the foldings ere the Cretaceous beds were deposited, as also the relations of the outcrop of the Jurassic strata to the approximately horizontal surface. Then taking a contoured map of the country under investigation, and obtaining the present height of the base of the Cretaceous in different localities, it will be seen that it is possible to follow the direction of the ridges and troughs produced by post-Cretaceous movements, the result of the application of these methods being to prove the important fact that the post-Cretaceous folds are exactly superposed on the pre-Cretaceous: this result, the professor believes, will also hold true when the Jurassic and Palæozoic strata shall have been more closely considered.

His general conclusion (p. 146) is that folds are always formed on the same spots, and that a general design, marked out already at the beginning of geological time, has presided over the deformation of the earth's crust.

Our author, however, does not consider that folds alone are sufficient to account for all the varied features of the earth's surface. He is of opinion that, together with them, there are elliptical portions of its crust which undergo eleva-

tion and depression, such movements not being continuous on the same spot (as with folds), but ofttimes developing more rapidly than folding, and that these areas of movement, or, as he terms them, Domes, are the originating cause of most normal faults, which are therefore due as much to elevation as depression.

The year 1892 has been conspicuous in the annals of tectonic geology, the works of Lapworth, Bertrand, and the publication of the second edition of Suess' *Antlitz der Erde*, marking prominently the direction along which the tendencies of geo-tectonic conceptions are proceeding, and in this same year Bertrand (14) reviewed mountain structure as a whole.

Recognising mountains to be folded zones of the earth's crust, he lays special stress upon the continuity of such regions throughout the old continent, and argues that they are the result of the crushing of wedges of the earth's sphere. He notes that in Eurasia three marked regions or belts have been traced out, the most ancient being the one nearest to the pole; of these belts, in the most southerly (the Alpino-Himalayan), even the Miocene strata have been affected by the movement; south of the line of coal measures, extending from Wales to Westphalia, is a second belt, in which all the palæozoic rocks are folded, and, north of that same line, a still older one, in which only the pre-Devonian have undergone that process. He therefore concludes that the oldest zones are those nearest to the pole, and, from the discordance observed in the most ancient series of rocks in the northern regions, is of opinion that it was in the polar regions that occurred the first dislocations of the earth's crust, and that at a period of time, possibly, preceding the appearance of life upon the globe. He is further of opinion that the folding movement, which has been active throughout long periods, has been gradually shifting towards the equator, the most recent chains due to this movement forming an almost continuous belt around the globe, whilst the more ancient form a series of roughly concentric belts approaching nearer and nearer to the pole. He recognises, however, that, whilst these

conditions appear so well marked out on the northern hemisphere, they fail hitherto to explain the character of the southern.

Discussing the question of faulting in its relation to folding, he points out that faults are most frequent in the great plateaux, and in feebly undulated beds; and considers that these arise from the parts insufficiently supported from below yielding under the influence of gravitation; and that the relative play of the component parts, and the deeper sinking of some of them, may be referred to the influence of centripetal action arising from the secular cooling of the planet.

Again, in zones where folding is active, faults, if produced, are a resultant of that action, and it is along their lines of fracture that pressure continuing induces sliding; but only from below upwards, thus causing the superimposition of the older beds on the younger. These faults are therefore inverted, and in so far as they affect mountains, only occur in the exterior or sub-Alpine zones. On this hypothesis, therefore, the fold is the element, the principal phenomenon, and the fault a detail arising from its action. In the central parts of the chains, however, where cohesion is too strong to allow of faulting along any other line than that of stratification, gliding and slipping has only occurred along the lines of least resistance; the softer beds being crushed out, the harder ones would retain an apparent conformity in their stratification, but the general effect of the combined movements would be the thinning out of the whole series. It will thus be seen that in so far as it regards the horizontal displacement of an overlying fold, its being drawn out under the influence of continuous pressure, and the crushing and thinning out of the beds of the inverted arch, when the latter is completely superposed on the strata forming the trough, in all these points Bertrand is in complete accord with Heim.

The various theoretical deductions, however, which during the last fifteen years have so greatly influenced geological thought, have not passed unchallenged, and even

this year have been submitted to a searching analysis by Dr. Rothpletz (15 and 16).[1]

We do not propose to review in detail the former of these two works, or recite the various arguments he adduces in it, these being more concisely formulated in the second one (read at the late Congress in Zurich), which lead him to conclude that many of the theories advanced to account for mountain building have failed to explain to his satisfaction the phenomena produced; suffice it to point out that, in his opinion, they fail to explain Helmert's researches on the diminution of mass under mountain chains, and that most of the effects could equally well be the result of expansion as of contraction, by means of the radial and tangential pressures that would be set up.

Dr. Rothpletz, however, carefully guarding himself against accepting expansion as the solution, throws it out as a suggestion, seeing that he considers the theory of contraction has failed in one important particular, and that it is necessary to seek other bases more in accord with the facts known to us.

Summarising the conclusions he arrives at in his latest work, in which he has dealt with the whole question of overthrusts, we find many old views discarded, and new ones suggested.

On the first point, which will perhaps find very general acceptance, he formulates :—

(1) All overthrusts are the accompaniments of foldings and mountain elevation, and are the result of the same forces acting more or less horizontally, that is, tangentially to the earth's surface ; and that where tangential pressure is still active, it produces in local and somewhat sharply bounded regions compression and folding.

(2) That overthrusts in general have a strike closely parallel to the folds, and their dip, though, as a general rule, it is towards the mountain region, may occasionally be from it. In Daubrée's experiment (fig. 2) it will be seen

[1] This, pamphlet, read before the International Congress at Zurich, has only just been published, and I have been indebted to Professor Sollas for the loan of an advance copy.

that a rock prism submitted to lateral pressure undergoes elevation, and that the portion which passes outside the region of compression ceases to be acted upon, and apparently expands, giving rise to an overthrust on the surface, but at the same time fault planes are produced, along which the overthrusts are formed, having their dip inwards. This, therefore, he considers explains, not only the overthrust of the older beds over the younger, but also the occurrence of such on both sides of a mountain chain with their dips in opposite directions. Owing, however, to the very variable character of the rock masses, the simplicity of fracture displayed in this experiment is complicated by minor movements tending to hide the facts of development.

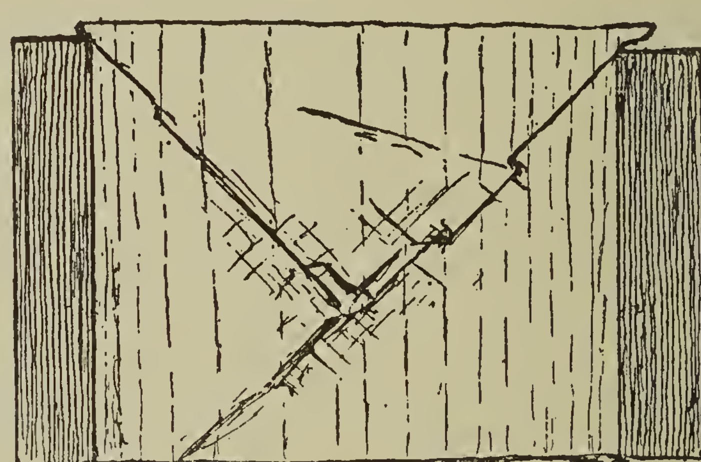

Fig. 2.—Illustrating the production of overthrusts along fault planes formed during lateral compression, as displayed in Daubrée's experiment.

(3) Although generally overthrust planes have the same strike as the folds, yet in some cases slight divergences from absolute parallelism occur. These are known from all regions, and show that though both are originated by the same forces, the folds were produced earlier, and the change of direction in the formation of the overthrust arises from increased compression caused by augmentation of the pressure action.

(4) The dip of the overthrust planes almost invariably differs from that of the strata or folds, sometimes merely cutting across a trough or saddle at an acute angle to the axis plane; and at others so obtuse to that plane that they pass direct from an arch into the adjoining trough. This proves, therefore, that the effort which produced them is

independent of the fold movement. Though there may be isolated cases of absolute correspondence of the overthrust surface and the axis plane, yet the examples where the other conditions prevail are so much more numerous that they should be considered as exceptional and not in essential relationship.

(5) As a rule the strata both above and below the thrust plane are normal—that is, the younger overlie the older. When the thrust plane has a very feeble slope, and stratification is simple, the result is the repetition of the same series of strata; should, however, these be much dislocated and compressed into many folds, they are, apparently, more complicated, and the series may be repeated several times where folds are very inclined, lying sometimes on the thrust plane in a normal, and sometimes in an inverted, condition, this latter being invariable when the lower limb of a lying trough has been overthrust by the upper one. In cases where the thrust distance has not been an extended one, or the direction of thrust has been at a highly-inclined angle, the tectonic relations of the troughs and saddles are not completely obscured; but where the overthrust has extended several miles, and exceeds the breadth of the folds, and still more so, should the thrust plane be horizontal, then the original tectonic relationship will be scarcely recognisable, and the nature of the strata below and above the overthrust may be completely different in character.

(6) The rocks bordering the overthrust plane have in almost every case undergone mechanical and chemical alterations of an intense character. This ordinarily consists in the formation of coarse breccias or fine sands through disintegration of the rocks. Owing to chemical solution and recrystallisation, the breccia rock may differ markedly from that whence it was derived, polished planes and cleavage being produced, giving rise to structures such as fault rock, mylonite, etc., which are, all of them, more or less parallel to the thrust plane.

These structures may also, in many cases, be easily recognised as having been produced through the grinding of the constituents along a common friction plane, as when two

mountain masses are moving in opposite directions, and generally, recognisable fragments of the neighbouring rock are disseminated throughout the breccia ; in some cases there is gradual transition from the one to the other, whereas in others, the boundary is sharp, or the breccia may be intruded into the parent rock in a series of tongue-like projections.

(7) The overthrusts often follow but one fault plane, though more commonly they have taken place along several, giving rise to a step-like formation (the Schuppen structure of Suess). It is possible, therefore, for a single overthrust to be broken up into a number of small ones, and *vice versâ*. We have also examples wherein the greater cut off the less, the former being known in the Scotch survey as major, and the latter as minor overthrusts.

(8) Though it apparently holds true that in every mountain chain overthrusts have occurred, in many cases they have not been abundant, their place being then taken by vertical faulting, due to the elevation of one side of the fault, this being the result of the same act of compression which produces the phenomena of overthrust ; this condition is conceivable if movement coming from one side only should have resulted from the compression of the deeper earth-mass.

(9) It is difficult, however, under these circumstances to sub-divide faultings into (1) heaves, in which, by the means of pure tangential forces, horizontal movement alone without vertical displacement has been produced ; (2) faults, due entirely to the action of gravity ; and (3) overthrusts, due to a combination of tangential and vertical forces. In faults having a hade, the direction of the movement may be easily traced ; but in such as are perpendicular it is difficult to determine whether the present position of the lower-lying beds has been reached through a sinking on their side of the fault, or is due to elevation on the other. Thus, in the Eastern Alps, troughs have been broken up in such a manner that the base appears to overtop higher portions of the trough limb, and it is uncertain whether it has risen, or the other sunk with regard to it (see fig. 106, *Geo-Tecktonische Probleme*). Similarly, the central part of an

anticlinal arch often appears to have sunk as regards its outer portion, but it is difficult to say whether elevation or depression has been the actual cause.

An appeal is made by the author that the conclusions should be tested on the basis of ascertained facts, and not on theoretical considerations, which, he maintains, should follow, rather than lead, the interpretation.

Applying his deductions to a consideration of the Doppelfalte (the central citadel of those holding overthrust to be overfold), he maintains that there is no evidence whatever of the existence of northern and southern return foldings, all the strata represented on Heim's figure (see fig. 1) in dotted lines being absent, while the Jurassic appears in the Rhine valley far north of the position of the point of outcrop necessary to support his theory.

Again, rejecting the theory of the northern middle limb, he argues that far from the Verrucano covering the Lochseitenkalk (representing the rolled-out Jurassic) at every spot along the thrust plane, there is in reality a series of broken folds of Jurassic and Verrucano strata at various points overlying the thrust plane, and adds that for the existence of the southern limb of the southern saddle there is no evidence whatever. Therefore the supposition that these members were present, but had disappeared through mechanical deformation and erosion, is purely hypothetical, and only advanced to explain stratigraphical relationships.

This, however, he considers unnecessary, believing that these relationships explain themselves simply; a highly-folded Eocene synclinal has been folded over from the south, and, from the north, older rock has been thrust over it along a very slightly inclined plane (see fig. 3), and further, that to the south of this synclinal the beds still in normal position have been trough-faulted, giving rise to the broad Rhine valley.

He is, therefore, of opinion that the hypothesis of double folding, with the squeezing out of the middle limb, is not only unnecessary, but opposed to facts. The appearance of longitudinal and transverse faults in many parts of the country is entirely foreign to the conception of folding

having taken place in the region without the production of faulting. (It may be here pointed out that Dr. Rothpletz argues that the Linththal is the result of a broad mountain region being faulted down between two parallel fault planes, whereas Heim holds that all the principal valleys are the result of continuous erosion acting through long periods.) The peculiar folding of the rock-mass, which has been pushed forward from the north, and cut off by the thrust plane, is inconsistent with the conception of a squeezed out middle limb (*Ost. Alpen*, p. 256). What, then, are the great principles which underlie this attack upon an apparently well-established theory? Folding, as the great basis of mountain building, remains unaffected, but that

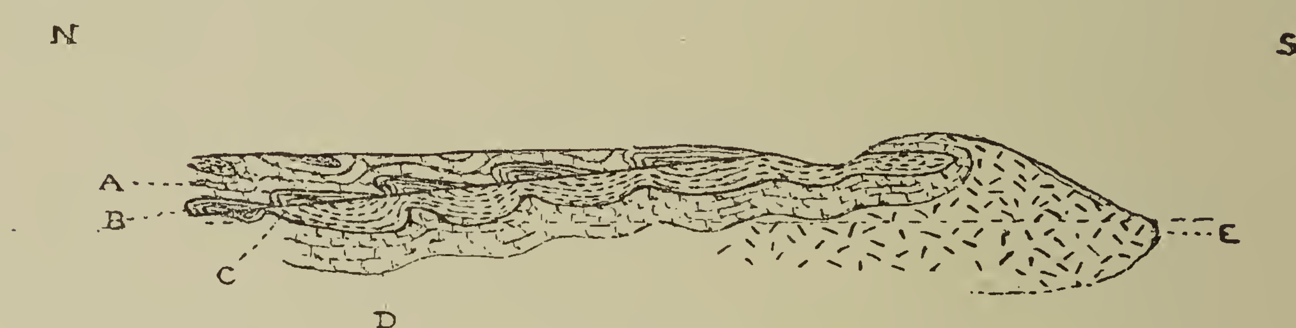

FIG. 3.—Illustrating Rothpletz's interpretation of the Doppelfalte.

A the Jurassic, and B the Verrucano members of the broken folds pushed along the overthrust plane, which is here represented by a dark line.

C. Eocene trough, contorted as in previous figure. (The curves have been simplified in the drawing.)

D. Jurassic member of the inclined trough.

E. Verrucano member of the inclined trough.

overthrusting is synonymous with overfolding is strenuously denied. The leaders of the New Geology have hitherto, to a large extent, held that folding and overthrust stand in the closest connection, whereas Rothpletz maintains that folding has preceded the overthrust, the latter frequently cutting right across the folds.

On the other hand our author considers that a close relationship exists between overthrusts and faults, and endeavours to show that where the former are scarce, normal faulting takes their place.

The points in Heim's theory that he specially attacks are: (1) The overfolding with squeezing out of the middle limb; (2) the conception of latent plasticity; (3) rock meta-

morphosis without faulting, and (4) he strongly insists that many of the principal Swiss valleys are the production not so much of erosion as of faulting.

In support of these views Dr. Rothpletz, in his latest work, has gathered together evidence from all the principal mountain regions. In the Linththal, opposing the view that folding has originated in the deeper-seated portions of the earth's crust without faulting, he maintains that great dislocation fissures occur which extend downwards into the deepest strata open to observation.

He also points out that where, according to the Doppelfalte theory, Jurassic Hochgebirgskalk was supposed to exist, in reality, the whole precipice consists of all strata from Eocene to Dogger, and instead of a simple inversion having here taken place (south fold of southern saddle, where the squeezed out middle limb is shown overlaid by Verrucano), the whole is folded into a series of troughs and arches, the Malm being in places folded three times on itself, sometimes enclosing Dogger and sometimes Eocene beds.

He describes a series of vertical to overturned folds in the Sentis region, cut through by inclined longitudinal fissures, along whose fault planes the upper side has been thrust over the lower. These overthrust planes follow in general the longitudinal direction of the folds, but they are never absolutely parallel with the strike of the strata, being mostly of greater inclination than the latter.

The overthrusts are clearly younger than the folds, cutting sometimes across the highly inclined, and at other times through the horizontally lying fold limbs, having no definite relation which would prove them to have been derived from the overfolding of the same. On the other hand the fault planes are clearly younger than either, and along them horizontal displacements and depressions have taken place, giving rise to all the varied mountain forms of the Sentis range.

In the Juras we again learn on Muhlberg's authority that the folds and overthrusts have not the same strike, and that the latter could not have been sufficient to squeeze out a middle limb 800 metres in thickness (this figure represent-

ing the amount of strata which would have to be affected by it). In addition the beds both above and below the thrust plane dip at a higher angle than the latter, and nowhere along the surface does any evidence exist of a squeezed out middle limb.

To account for these conditions our author again refers to Daubrée's experiment (fig. 3, and *Geo-Tecktonische Probleme*, fig. 41), submitting that instead of these great inversions originating under deep-seated conditions, they have in reality been formed in that part of the earth's mass no longer subject to continued compression. The occurrence of overthrusting only to the north of the Juras is accounted for, if these mountains be included as forming part of the Alpine system, seeing that the whole movement lies in the northern direction of the northern side of the main mountain axis, though exceptional conditions may arise, such as the southern overthrust in the Glarner Alps.

As additional evidence in support of his theory Rothpletz cites the Scotch area, and quotes from the *Report of the Scotch Geological Survey*, p. 411.

"There are two points, however, in the former official report, which, in the light of recent evidence, require modification. First, it was stated that during the incipient stages of the movements the strata were thrown into folds, which became steeper along the western front till they were disrupted, and the eastern limb pushed westward. The folds were believed to have culminated in reversed faults; but it is apparent that the latter need not necessarily be preceded by folding."

It is a remarkable fact with regard to Scottish thrust planes that not a single case of inversion of strata seems to be known, rendering, therefore, almost impossible any connection with original overfolding, and the suggestion is hazarded that lateral pressure having been active, the elevated mountain area has been thrust over a lower-lying plateau which occupied the region now hidden beneath the sea between the mainland and the Hebrides.

The Lausitz (Saxony), Westphalian, and Belgian areas are next considered, and although in the latter the results

are more highly complicated, owing to the original folding having been much masked by subsequent faulting, yet in the main the same general lessons are to be learnt. Rothpletz, in dealing with the district of Provence, lays special stress upon the fact that here also there is no absolute verification of the "squeezing out of the middle limb" theory. He considers that without its aid all the profiles may be explained, and that, in view of this uncertainty, it would be advisable to reject the term *pli-faille*, substituting for it one without theoretical signification, such as *pli de recouvrement.*

It cannot be doubted, however, that in this part of France folding and overfolding of the strata have been antecedent, and that subsequently and locally the folded masses have been thrust over each other along fracture surfaces inclined at a low angle; also most of the dislocations and depressions have taken place along fissures, which have affected both fold and overthrust.

Finally, he remarks that long since in America Professor H. D. Rogers had recognised the fact that overthrust could take place along a fault plane developed parallel to the axis plane of an anticlinal fold.

A review of the whole subject of mountain building reveals to us the existence of a number of conflicting opinions, attempting to explain by various methods a series of well-ascertained facts.

Even the main theoretical basis is by no means definitely settled, or universally accepted. Thus, while the greater number of observers are agreed to consider the slow cooling of the earth's surface, and its consequent contraction, as the chief cause of its present broad features, others, notably Mr. Mellard Reade, hold the view that expansion has played the most prominent part; that in regions of depression, where accumulation of sediments is continuing, there is a gradual rise of the isogeotherms as we descend; that from the expansion produced by this rise upper curvature of the underlying rocks will eventually be produced; and also that from the effect of this expansion on the overlying beds vertical movement will result, giving rise to the formation of mountain chains.

Putting aside, however, any further discussion of the views held by many most competent observers, we would, in conclusion, place in as sharp a contrast as possible the two broad principles which we have endeavoured to review.

The first, originally propounded by Heim, may be summarised : mountain structure is the direct result of folding. This has taken place far beneath the earth's surface, the rocks affected being, under the influence of enormous pressure, in a condition of latent plasticity, consequently faulting in the mass becomes impossible. Overthrust is the resultant of extreme overfolding, the middle limb of a fold being drawn and squeezed out between the arch and trough. Subsequently, erosion, having removed the great thicknesses of superjacent rock-masses, has revealed the folding, and at the same time caused the varied features of the mountain chains.

The second, of which Rothpletz is the exponent, formulates : folding is the first step in mountain building as a result of lateral pressure, but, as in Daubrée's experiment (fig. 3), on upheaval, part of the mass ceases to be affected by such pressure, great fault planes are produced, along which parts of the *already folded* beds are thrust over the younger ones. Overthrusts, therefore, are really faults of a special type ; and the more normal form succeeding them determines the formation of valleys and peaks, lakes and precipices. The first connects overthrust with folding, the second with faulting. The one demands enormously thick overlying deposits and erosion acting through long periods ; the other, no special conditions, and attributes the present features of the earth's surface mainly to faulting.

In the *Livret Guide*, issued to the members of the late International Geological Congress in Zurich, the Swiss geologists have endeavoured to explain the inner structure of the Alps, and no one can fail to be struck with the importance which the character of the deposits has had on the nature of the rock-foldings. In the section illustrating Schardt's Alpine excursion the *failles de recouvrement* are seen both to the north and south of the Pays d'Enhaut to have formed between the Eocene shales and the

Cretaceo-jurassic limestones, the former being highly contorted and underthrust, while the Jurassic limestones overlap them on both sides.

The questions demanding solutions are therefore of the most complex character; and only careful research in each mountain district, together with the widest application of mapping and physical and microscopical investigation, carried out by careful and indefatigable observers, can reveal the true interpretation of the facts with which we are now acquainted. It may be that as yet it is only grasped in part, and that the key to the tectonic problems has yet to be found; but by the conscientious working out of details a body of facts may be accumulated which will enable geologists in the future to determine the full significance of those phenomena whose effects are revealed to us under such varied and impressive forms.

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W. F. HUME.

NEOZOIC GEOLOGY IN EUROPE.[1]

TEKTONIC GEOLOGY.

IN an article which appeared in "SCIENCE PROGRESS" for June, a short account was given of the views of Bertrand on the structure of the French Alps. Since that article was written M. Bertrand has published his results in greater detail, and has plotted upon a map the anticlinal axes of the principal folds in this region (1). In general, the axes run from north-east to south-west, or from north to south, but in detail they are very sinuous. Even without taking into consideration the "amygdaloids" or "kernels" already described ("SCIENCE PROGRESS," vol. i., p. 323), there are a series of bends visible at many points. The direction of the river-valleys is closely related to the direction of the folds, but instead of running, as we might expect, in the lines of the folds, they run perpendicularly to those lines; and this is especially noticeable at the sinuosities. Bertrand attributes this peculiarity to the presence of a transverse system of folds, which is less important than the longitudinal, yet, on account of the isoclinal character of the latter, has a greater effect in determining the direction of flow.

Of the sinuosities in the lines of folding, by far the most striking are those around Petit Mont Cenis and the Grand Paradis. The axis of symmetry of these sinuosities corresponds with the valley of the Durance; and Bertrand points out that if we prolong the line of this valley above Guillestre we are led to the eruptive mass of Mount Genèvre, and thence to a series of similar sinuosities in the

[1] The term Neozoic is used in the sense in which it was originally proposed by Forbes—to include Mesozoic and Kainozoic. The present article is the direct continuation of the article on "Mesozoic and Kainozoic Geology in Europe," which appeared in "SCIENCE PROGRESS" for June, 1894.

folds on the Italian side of the Alps. There is in fact a kind of pinching in of the chain on this transverse line, and the folds of the two flanks approach each other. Moreover, as the various rock-zones of the Alps are traced from the north towards this line, they sink down below the surface (with the exception of the subalpine zone). Thus the line corresponds with a general transverse lowering of the whole of the northern part of the chain; and it is approximately parallel to the axes of folding in that portion of the range.

Bertrand believes that this feature is due to a large synclinal belonging to the same system as the folds to the north; and he looks upon the plain of the Po and the depression of the Canal du Midi in Languedoc as the easterly and westerly continuations of this depression. He believes, in short, that in Palæozoic times the chain of the Alps did not bend round, as it does now, to join the Apennines, but that it was continued with its original direction towards the Montagne Noire in Languedoc. The southern part of the chain is of later date, and the direction of the folds which produced it, is different.

The folds in the Alps to the north of the region investigated by Bertrand have been described by Haug (2), and from his account it appears that the axes of the folds do not run exactly parallel to the chain, so that as we pass along any one orographical zone we cross slowly from one system of folds to the one lying behind or in front of it.

Still farther to the north the structure of the Chablais has again been described by Lugeon (3). His views, however, have already been noticed in a previous article.[1]

In the Eastern Alps we have at last a section across the whole chain, from Tölz in the south of Bavaria to Bassano in the north of Italy. We owe it to Rothpletz (4), and it is the first complete section which has been attempted since the *Geologische Durchschnitt der Alpen von Passau bis Duino*, published by von Hauer so long ago as

[1] "Science Progress," June, 1894.

1857. It would lead us too far to discuss this important work as fully as it deserves; but it is necessary to notice one or two points on which the views adopted by Rothpletz are by no means those which are generally accepted. On the much-debated question of the origin of the Schlern dolomite in Southern Tyrol, he sides with those who of late have attacked the views so strongly advocated by Mojsisovics. He believes that this great mass of dolomite owes its formation to algæ and not to corals.

On another question of general interest he has much to say. He discusses the work of Heim on the Glärnischer Alps, and comes to the conclusion that the double fold which figures so prominently in our text-books has no existence. The Lochseitenkalk which forms the middle limb of the northern fold in Heim's section, and which Heim believes to represent the Jurassic rocks dragged out, is according to Rothpletz nothing but a fault breccia, with a certain amount of vein material. The plane along which we find the Lochseitenkalk is a great thrust plane.

The most important result of a change of view of this kind is that it reduces very considerably the amount of contraction which it is necessary to suppose that the Alps have undergone. According to Heim,[1] in the Northern and Central Alps the beds which, folded as they now are, occupy a breadth of 82 kilo., would, if flattened out, spread over a width of 158 kilo. Rothpletz arrives at a very different result. The actual width of the Alps in his section is 222 kilo.; while, if the beds were unfolded, it would be 271·5. Thus the contraction is only 18 per cent., instead of nearly 50, as Heim's figures would give it. The two writers are treating of different areas, but Rothpletz believes that Heim's result is due to error in his interpretation of the folds.

The earth-movements which gave rise to the Alps took place at various periods. They certainly began in pre-Permian times, and continued at intervals until at least the Miocene period. In the Western Alps, near the

[1] *Mechanismus der Gebirgsbildung*, vol. ii., p. 213.

great bend of the Rhone above Geneva, Golliez (5) has brought forward evidence of pre-Carboniferous folds. The "cornes vertes" in the Dent du Mercles strike N.N.E. to S.S.W., and the Carboniferous rocks rest upon their upturned edges.

Leaving the Alps we must notice here a second paper by Bertrand (6) upon the various systems of folds throughout France. He still maintains the two laws which he has already attempted to prove; *viz.*, (1) that when once a system of folding is established, subsequent folds tend to be formed along the same lines; (2) that, in France at least, in spite of various sinuosities, the lines of folding form an orthogonal network. On the map which accompanies this paper the most conspicuous of the two systems of folds has a general east to west direction; but the lines, instead of being straight, are slightly curved, the concavity facing the north. This concavity increases as we proceed from the north to the south, and towards the Central Plateau it becomes double. The westerly concavity spreads down into the department of Ariége; the easterly again bifurcates, extending on the one hand into the valley of the Po, and on the other into Provence and the Alpes Maritimes. The second system of folds lies nearly at right angles to the first, but is very much less conspicuous.

As to the mode of formation of folds, Zürcher (7) has made some observations which are worthy of note. He believes, no doubt correctly, that a fold is not formed along its whole length at one time, but that it originates at one point and gradually spreads in two opposite directions from that point. And he concludes, on perhaps somewhat imperfect evidence, that the point where the folding is now most intense is the point where it first began to be formed.

Among papers dealing with special areas there is one by Tardy (8) upon the faults and folds in the Jura, which is, however, very brief and general in its statements. In the Corbières in Languedoc Grossouvre (9) has proved the existence of a thrust plane which he traces from St. Louis to Corbières, and which lies on the north side of the anticlinal which bounds the Albian basin of St. Paul de

Fenouillet. Lorenzo (10) describes in detail the folds in the neighbourhood of Lagonegro in the province of Basilicata in the South of Italy. The anticlinal and synclinal axes run, as might be expected, nearly north to south.

Salt deposits of the Basses Pyrénées.—In the South-west of France, near Bayonne, there are a number of salt-bearing deposits which have been the subject of considerable discussion. According to Seunes,[1] who has described the area in some detail, they belong to the Trias; but this view, which is based on their resemblance to rocks of known age in the Pyrenees, has not been universally accepted. Dufrenoy found Cretaceous fossils in them; and Macpherson has shown, by microscopic examination, that the Cretaceous marls of the neighbourhood are continuous with the "glaises bariolées" or salt-bearing beds. According to Gorceix (11) the salt deposits are all in close connection with the ophitic outbursts of the area. These ophitic outbursts took place along a number of lines which radiate from a point three kilometres west of Labastide Clairence, and which he believes to be faults; and the salt-bearing deposits occur upon these lines also, generally in fact where the ophite is exposed. When the Cretaceous Flysch occurs close to an ophitic outburst it is converted into "glaises bariolées," and he believes that the "glaises bariolées" served as basins in which the salt water of the sea collected. The salt water on evaporation, in which it was no doubt assisted by the warmth of the eruptive material, gave rise to the present salt deposits.

TRIAS.

The Triassic system in general has been made the subject of an interesting paper by Wohrmann (12). One of the conclusions at which he arrives is that the sediments of the German and Alpine Trias were laid down in connected seas and not in separate and distinct basins. He finds more species common to the North and South Alps than has generally been supposed. The most considerable eleva-

[1] *Ann. des Mines*, ser. 8, vol. xviii., pp. 209-458, 1890.

tions in the floor of the sea were the "vindelician" and Central Alpine ridges. The former stretched from Bohemia in a south-westerly direction towards the Schwarzwald. It made its first appearance in the Muschelkalk period, and reached its greatest development during the deposition of the Lettenkohl beds (Lower Keuper). The ridge of the Central Alps was in existence in Permian times, and became less prominent towards the end of the Keuper.

Concerning the value of zone fossils Wohrmann seems somewhat sceptical. He remarks that cephalopods do not lend themselves any more than other forms to the exact determination of single horizons; for they also are limited to certain facies, and disappear and reappear with those facies. He lays great stress upon "international" forms, that is, forms of wide distribution; because a wide distribution implies indifference to local conditions—or facies. After all, the ammonite palæontologist does not contend for more than this; but he believes that ammonites are international forms.

In another and a much more detailed paper (13) Wohrmann has given an account of the Raibl beds (a part of Mojsisovics' Carinthian) throughout the length and breadth of their extent. The term is one which has been used in various senses, or rather with various limitations. It was originally employed by Foetterle and von Hauer to designate a series of marly and calcareous beds which at Raibl lies between the ore-bearing limestone and the Dachstein limestone. Palæontologically, this does not form a convenient division, for the St. Cassian fauna still lived on in the lower part of the series, and it was in the middle of Raibl times that the invasion of foreign forms gradually drove out the earlier fauna. But the change was not a sudden one, and no definite palæontological line can be drawn; while lithologically, von Hauer's grouping is the most convenient, and is adopted by Wohrmann.

The following table[1] will show the general classification

[1] This table does not show all the elevations and depressions referred to in the text, but only those which are indicated in the Franconian deposits, as well as in the Raibl beds themselves.

of the Raibl beds, according to Wohrmann, and their relation to contemporaneous deposits in Germany:—

	Swabia and Franconia.	North Tyrol and Bavaria.		
Lower Keuper.	Lower Gypskeuper. Very few Muschelkalk species.	Gypsum and Rauhwacke or Ostrea limestones. Very few Cassian species.	Torer beds.	Raibl beds.
	Unimportant elevation.			
	Grenzdolomit.	Lower limestone bank.		
	Depression.			
	Lettenkohl group with a Muschelkalk fauna; clastic sediments with plant remains.	Cardita beds, with a Cassian fauna in the lower part and a mixed fauna in the upper; sandstones with plant remains.	Cardita beds.	
	Elevation.			
	Upper Muschelkalk.	Wetterstein Kalk.		

Immediately before the Raibl period a great amount of deposition took place, mainly owing to an extraordinary development of marine algæ, and the result was the formation of the Wetterstein and similar limestones. This was followed at the commencement of Raibl times by a general upheaval, which in the south was accompanied by volcanic eruptions. Sandstones were laid down along the vindelician ridge, while further to the south the deposits were more marly. In the Eastern Alps the corresponding beds are slaty, and the fauna is poor: neither the character of the fauna nor of the deposits shows the influence of neighbouring land, and we may conclude either that no rivers flowed in this direction from the Bohemian massif, or that the coast lay farther to the north than in the Western Alps.

After the deposition of these littoral sediments a sudden sinking took place, and limestones and dolomites were laid down, forming the middle division of the Cardita beds. It is in the neighbourhood of the vindelician ridge, where the

littoral character of the earlier deposits is most marked, that the contrast is most clearly seen. In the Southern Alps no definite line of division can be recognised.

This was followed by another upheaval, a more considerable one than the former; and it also was accompanied by volcanic eruptions in the south. The vindelician ridge rose again, and the Bohemian mass grew out towards the south. Marshy islands formed along the northern border of what is now the Austrian limestone Alps, and upon these islands grew the plants which are found in the Lettenkohl group.

Again a sinking took place, and the Torer beds were laid down both in the Southern and the Northern Alps. They are mainly calcareous and dolomite.

All these elevations and depressions appear to have been simultaneous throughout the area.

It has hitherto been held that in the lower division of the Raibl beds there were scarcely any fossils common to the North and the South Alps, but Wohrmann finds a large number of species which occur in both. As for the Torer beds, it has always been recognised that these were very much alike on the two sides of the Alps.

In an important paper on the neighbourhood of Lagonegro, Lorenzo (10, see also 14) has given us the final results of his examination of that region. The Upper Trias is represented by the following groups (in descending order):—

Haupt-dolomite with *Gervillia exilis*, etc.
Dolomite Limestones.
Siliceous schists with Radiolaria.
Limestones with siliceous nodules. *Halobia insignis*, etc.

The lowest group corresponds closely with similar beds in Sicily, which have been referred by Gemmellaro to the zones of *Trachyceras Aon.* and *T. Aonoides.*

The succeeding series is principally characterised by the abundance of Radiolaria, such as *Porodiscus*, etc. The dolomite limestone contains numerous Gyroporellas of the group annulatæ, *Posidonomya*, *Daonella*, etc.; and from its cephalopods is correlated by Mojsisovics with the zone

of *Protrachyceras Archelaus*. Finally, in the Hauptdolomite, all the fossils, except the abundant *Gervillia exilis*, are Raibl forms.

The other papers on the Trias may be dismissed in a few words. Kittl (15) has come to the conclusion that the Marmolata and other allied limestones of Northern Italy should he referred to the zone of *Trachyceras Reitzi*, and not to that of *T. Archelaus*, in which Mojsisovics placed them. He unites them with the Buchenstein rather than with the Wengen beds.

Bertrand (16), who until recently believed that the "schistes lustrés" of the French Alps were pre-Triassic, has now reverted to the original view of Lory that they are really of Triassic age.

A controversial paper by Bittner upon the Alpine Trias requires no more than mention for the present (30).

JURASSIC.

Among the Jurassic rocks there is but little to record. The fourth volume, by H. B. Woodward, of the very important work on the Jurassic rocks of England and Wales, has been published by the Geological Survey (17). But, from the nature of the case, it does not lend itself to the purposes of the present article. Attention may be directed, however, to the description of the various kinds of limestone. Many of the oolites have been formed by algæ of the *Girvanella* type, but this mode of origin is not admitted for all.

Abroad we have a note upon the Liassic limestone of the Oisans (Dept. Isère) in the French Alps (18); and another upon the boulders of East Prussia, with a description of a section at Popiliani on the Windau (S. of the Riga Sea), from which neighbourhood it appears that these boulders may have been derived (19).

CRETACEOUS.

The most remarkable of the Cretaceous deposits is certainly the chalk, which is indeed one of the most peculiar formations met with in any geological system. For many

years it was universally considered to have been formed in deep water, like the Globigerina ooze of the Atlantic floor; but this view received a severe shock when Gwyn Jeffreys stated that, from a consideration of the molluscan fauna, he would infer that it had been laid down in shallow water.[1] Since that time opinion has been much divided, and the evidence of the fauna has frequently been discussed. The subject has recently been approached from a different point of view by Hume (20),[2] who has devoted much attention to the chemical and microscopical examination of the rock. He shows that the amount of terrigenous material in the Cretaceous beds gradually decreases from the Upper Greensand to the Upper Chalk, and from this he infers a gradual sinking of the land and of the floor of the sea.

Even from the general character of the Cretaceous deposits alone, we may conclude that, in the Anglo-Parisian basin, land lay towards the north in Northern Scotland, and towards the west in Ireland, the West of England and Brittany, for the deposits towards those areas are sandy; and, moreover, the upper beds overlap the lower towards the west. A relatively open sea lay over Kent and the East of England. More detailed examination shows that the same general distribution of land and sea existed throughout the Upper Cretaceous period.

Beginning with the Upper Greensand, which is largely of terrigenous origin, Hume points out that it is over 100 feet thick in Dorset, and that it thins towards the north and east, and dies out altogether at Dover and Rochester, showing that the land from which the materials were derived lay towards the west. From the abundance of glauconite grains, as well as the character of the fauna, he supposes that this deposit was formed at an approximate depth of 150 fathoms. It was, no doubt, more distinctly littoral towards the west.

In the case of the Lower Chalk, on the other hand, in which the calcareous material is in excess of that derived

[1] *Brit. Ass. Rep.* (1877). Trans. of Sect., pp. 79-87.

[2] See also Hume, *Chemical and Micro-Mineralogical Researches on Upper Cretaceous Zones in the South of England*, London, 1893.

from land, the thickness decreases from east to west. It is 198 feet thick at Dover, 116 feet in the Isle of Wight, and only 10 feet in Devon, and even this 10 feet is partly sandy. Moreover, as the thickness decreases, the proportion of insoluble residue increases: in Kent there is only some 15 per cent., in the Isle of Wight 40 per cent. Both these circumstances again point to the fact that land still lay towards the west and the sea had deepened.

In the Middle Chalk the amount of insoluble material is still further reduced. It has fallen to two per cent. in the Isle of Wight, and four per cent. at Folkestone. In the Upper Chalk the proportion of residue is equally low.

If, then, we may assume that the insoluble residue was derived from land, and the calcareous material was formed in the sea, we have clear evidence of a nearly continuous depression of the sea floor and recession of the coast line—a few small fluctuations there were, but these are unimportant. And that this assumption is justified is shown by the fact that the beds in which insoluble residue predominates thin out towards the east; while those in which the calcareous matter is most abundant thin towards the west. Moreover, as the insoluble residue diminishes, the heavy minerals, such as zircons, which must have been derived from land, become first of all smaller in size and then disappear, and the glauconite grains disappear also.

The depth of the Middle and Upper Chalk sea must, therefore, have been considerably greater than that of the Upper Greensand sea; and this view is supported by the decrease in numbers of the gasteropods and the proportionate increase of monomyarian lamellibranchs. Hume also bases part of his argument on the Foraminifera, and his final conclusion is that the Upper Cretaceous was laid down in a sea at least as deep as the Mediterranean.

In questions of this kind, when the argument is based upon the assumption that fossil forms lived and flourished under the same conditions as their recent allies, it must always be borne in mind that such an assumption is open to grave doubt. There is nothing to prevent an animal from adapting itself to live under a new set of conditions

(for instance, at a different depth of the sea) without becoming so much altered as to constitute a new genus. In this connection it is interesting to notice some recent observations of the Abbé Bourgeat upon the Gault of the Jura (21). From a comparison of the faunas at several localities, he finds that the gasteropods and cephalopods flourished best where sandy materials were being laid down; while lamellibranchs preferred the quieter waters which were depositing clay. Moreover, the ammonites, as if to enable them to resist the shocks of the material among which they lived, acquired tuberculate shells.

At the same time it is worthy of note than in the Barrêmien of Châtillon-en-Diois, Sayn and Lory have found ammonites in a reef deposit among a series of limestones, where the conditions must have been very different (22).

It is well known that the chalk facies of the Cretaceous system does not spread over the whole of Europe, and that in fact it is confined to the more northerly parts and to Russia. The Upper Cretaceous sea of Northern Europe appears to have extended from the Anglo-Parisian basin eastward between the old central ridge of the Alps on the south, and the ancient rocks of Scandinavia and North Russia on the north. It sent an arm southward through France, which probably united it with the sea of the South of Europe. But even in this northern sea by no means the whole area was occupied by chalk. As we approach the old southern shore, for instance, in Saxony and Bohemia, the Upper Cretaceous loses its chalky character and becomes sandy. Within the Alps themselves there are a number of small areas of Cretaceous deposits of a still more littoral character. These are known as the Gosau beds, and appear to have been laid down in narrow gulfs, which ran southwards into the old alpine land. The exact correlation of these deposits is a matter of considerable difficulty, and has given rise to much discussion. A new examination of the beds in the Gosau valley itself has been undertaken by Kynaston (23). According to him the series is constituted as follows (in descending order):—

Upper Gosau beds.	5.	Sandy marls, alternating, especially towards the upper part, with sandstones, grits and conglomerates.
	4.	Sandstones and flags, with some sandy shales.
Lower Gosau beds.	3.	Bluish-grey marls, with some limestone; very fossiliferous. *Hippurites organisans*, corals, etc.
	2.	Estuarine series of the Neue Alp.
	1.	(d) Limestone with *Nerinæa*.
		(c) ,, *Actæonella conica*, etc.
		(b) ,, *Hipp. cornu-vaccinum*.
		(a) Coarse conglomerate, with grits, sandstones and marls.

In the Gosau district itself these beds rest upon Triassic rocks, but near Salzburg they lie upon the Gault, and must, therefore, be of Upper Cretaceous age. It is not possible to make a direct comparison of the Gosau beds with those to the north, but the fauna is very like that of the Corbières in the South of France. Toucas there recognises two Hippurite zones, the first containing *Hipp. cornu-vaccinum* in abundance, and the second with *Hipp. organisans* and *H. cornu-vaccinum*;[1] and Kynaston correlates his two zones with these. Toucas places his first Hippurite zone in the Upper Turonian, and his second towards the top of the Senonian; and Kynaston concludes that the Lower Gosau beds represent the upper part of the Turonian and the whole of the Senonian, while Toucas correlates them with the Senonian alone. The age of the Upper Gosau beds, in which very few fossils are found, must still remain somewhat doubtful, but they may represent the Danian of other areas.

Of other papers upon the Cretaceous it will not be necessary to say much here. In the Lagonegro district only the Apturgonian is represented (10). It there rests unconformably upon the Lias, and consists of limestones. In the Monte Consolino, near Stilo in Calabria, Bassani and Lorenzo believe that the limestone which forms the crest of the hill includes beds later than the Tithonian (24). Sacco introduces some emendations in the geological map

[1] It should be remarked that Toucas does not attach much weight to the presence of *H. cornu-vaccinum* and *H. organisans*. Both of these occur in both zones. *H. dilatatus* and *H. bioculatus* appear to be the forms most characteristic of the upper zone. Toucas: *Bull. Soc. Geol. France*, ser. 3, vol. x., p. 200.

of the Northern Apennines (31). Finally, Stuart Menteath (25), in a series of controversial papers, has attacked the views of Seunes upon the geology of the western part of the Pyrenees.

TERTIARY.

The most interesting of the recent observations upon the Tertiary deposits are those which have been made in the eastern parts of Europe. In Roumania and the South-west of Russia there is an extensive basin of Tertiary deposits of which mention has already been made in these articles. On the general map of Roumania a part of this basin has been coloured as Eocene, and this colour has been extended to include a series of conglomerates towards the Transylvanian Mountains. These were naturally enough grouped together on a preliminary survey, but Stefanescu has shown that they are really of various ages. Some of them contain *Cerithium disjunctum*, and belong to the Sarmatian; in others *Cerithium minutum* has been found, and these must be referred to the Second Mediterranean Stage; while others contain nummulites, and have been correctly placed in the Eocene (26).

The Upper Tertiary beds (Miocene and Pliocene) occupy a larger space than the Lower (Eocene and Oligocene). The Sarmatian, for example, may be traced at intervals along the southern flanks of the Transylvanian Mountains in Wallachia, and covers the greater part of Moldavia (27). In general the sequence closely resembles that in the Vienna basin. In Austria, however, there is a distinct gap between the beds with *Cerithium* (Sarmatian) and those with *Congeria* (Pontian).[1] For a considerable time it was supposed that a similar gap existed in the Roumanian Tertiary. In Bessarabia, however, Sinzow discovered an intermediate fauna, and to the beds in which this occurs, Andrussow has given the name of méotique. They consist of yellow-green sands with limestones and

[1] The Sarmatian is placed at or near the top of the Miocene. The succeeding Pontian beds are referred by some geologists to the Miocene, by others to the Pliocene.

clays, and the fauna is partly marine, partly freshwater, or even terrestrial. According to Andrussow, at the end of Sarmatian times the sea formed a number of basins, the water of which, originally salt, gradually became brackish and then fresh. The marine fauna slowly gave way to freshwater forms; and towards the beginning of Pliocene times the basins had become large freshwater lakes (28).

A general account of the younger Tertiaries of Roumania is given by Fuchs (29). In the mountains which lie between that country and Banat, in Siebenbürgen, there are a number of isolated basins of Tertiary rocks. They are characterised especially by the occurrence of lignite-bearing beds, with *Cerithium margaritaceum* and *C. plicatum*. The best known of these basins is that of Bahna, north of Vercierova, in which, besides the Cerithium beds, normal Leitha limestones are found and marine marls with a fauna precisely like that of Lapugy. The Cerithium beds themselves have usually been correlated with the Pectunculus sandstones of Hungary (Oligocene); but according to Fuchs, excepting the two Cerithia mentioned, not one of the fossils is Oligocene, while all, including the Cerithia, are known to occur elsewhere in the Miocene.

It is remarkable that the lignite beds are never met with in Roumania, except in these isolated basins. They are unknown in the true Roumanian basin, where we have the following succession:—

(e) Unio beds. *Unio*, *Vivipara stricturata*, etc.
(d) Psilodont beds. *Unio*, *Vivipara Alexandrini*, *V. Heberti*, etc.
(c) Congeria beds. *Congeria stromboidea*, etc., *Unio*, *Vivipara*, etc.
(b) Sarmatian.
(a) Salt-bearing beds. *Ditrupa incurva* occurs in an associated Nullipore limestone.

The salt-bearing beds are correlated with those of Wieliczka, and are placed in the Mediterranean stage; the Congeria beds represent not only the Congeria beds (Pontian) of Austria, but also the Lower Paludina beds; while (d) and (e) correspond with the horizons of *Vivipara bifarcinata* and *V. stricturata* in Austria.

The papers dealing with Tertiary deposits in other areas will be noticed in subsequent articles.

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